Immigration, Integration, Public Perceptions, and Effects of Citizenship

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Judith Saurer

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Judith Saurer

Referent: Prof. Helmut Rainer, Ph.D.

Korreferent: Prof. Albrecht Glitz, Ph.D.

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"Ignoranti quem portum petat nullus suus ventus est."

Lucius Annaeus Seneca

Chapter 1

Opening Remarks

1.1 Introduction

In the last ten years around four million people have migrated every year into the OECD countries. After the U.S., Germany has become the second largest recipient of immigrants, just before the UK and Canada (OECD, 2017). In light of these mass migration flows, migration turned into one of the most salient topics in nowadays societies. Especially the humanitarian crisis due to the conflict in Syria is prominent in the public debate. Questions arise on how many people societies should host, how natives and governments will be affected and most importantly, how migrants are integrated into the host society.

The mass migration inflows into many developed democracies had furthermore a crucial impact on the public perception of immigration and its consequences in the political arena. Recent studies show that the most prominent political decisions in the year 2016, the Brexit vote in the UK and the 2016 U.S. elections, were driven by the public concerns about immigrants and the successful anti-immigrant rhetoric of some political parties (Klinkner, 2017; Harding, 2017). So far, empirical studies have analyzed the characteristics of individuals and their attitudes toward immigrants. Most prominent factors are fear of competition over scarce resources and culture. What is lacking is the causal evidence of how the geographic proximity of immigrants in one's region affects public opinions of immigration. I provide this evidence for Germany in Chapter 2.

A related topic to the public opinion of immigration is the integration of immigrants. Integration is a key component of the public perception and also of the social cohesion and welfare for both natives and immigrants. Still, we observe substantial performance gaps of immigrants in regard to natives. These performance gaps are reflected in economic terms (Algan *et al.*, 2010; Borjas, 1985; Chiswick, 1978) and educational terms (Dustmann *et al.*, 2012; Riphahn, 2003).

The educational integration of immigrant children has become a key priority for many governments, particularly because the youth population of immigrants is one of the fastest growing population segments. Already every third child born in Germany has a migratory background. Yet, relatively little is known about which interventions could be effective in

fostering immigrant children's educational integration. A successful integration in early childhood could lead to better occupational and social outcomes.

One highly debated integration measure is the granting of citizenship to immigrants. For children, the granting of citizenship at birth is a widespread public policy in many European countries. Literature has shown that birthright citizenship is effective in the integration of the parents of the migrant children (Avitabile *et al.*, 2013, 2014; Sajons, 2016). Chapter 3 and 4 of this thesis demonstrate also positive effects of birthright citizenship on the integration of immigrant children.

The remainder of this chapter is structured as follows. Section 1.2 digs deeper into the issue of public perception of immigrants and its relevance. Section 1.3 deals with the closely related topic, namely the integration of immigrants and especially immigrant children. The following Section 1.4 explains the possibilities of causal inference and the two most important features, exogenous variation and suitable data. Lastly, section 1.5 gives an outline of this thesis.

1.2 Public Perception of Immigrants

The public opinion toward immigration is a salient topic in many societies. It has the potential to change the societies on several dimensions: the political, the social, and the economic level. In what follows, I want to discuss the potential mechanism of the public perception of immigrants and how it could change the world we are living in.

The public perception of immigrants has already changed the political arenas of many developed countries. Many democracies have experienced shifts to a right-wing spectrum, for example, France through the Front National, Germany through the Alternative for Germany (AfD) or the UK Independence Party (UKIP) in Great Britain. Recent studies also show that current political decisions such as UK's referendum to leave the European Union and the 2016 U.S. election were based mainly on the public debate over immigration (Klinkner, 2017; Harding, 2017).

Public perception of immigrants has also some economic consequences. Negative public perception of immigration can have far-reaching effects in terms of countries' prosper-

ity and growth by influencing migration patterns and lessening business attractiveness (Siedler, 2011).

Negative public perception leads furthermore to social tensions between native and immigrant groups and could lead to parallel societies in the host country (Dustmann and Preston, 2001). The crucial underlying question in this context is how the actual presence of immigrants is an important factor in the formation of the public perception of immigrants. In Chapter 2, I address this question showing evidence on the public perception of immigrants during a major inflow of asylum seekers, namely during the Balkan Wars.

1.3 Integration of Immigrants

The integration of immigrants is a key policy challenge for domestic policymakers and beyond (OECD, 2017). Integration is important not only for immigrants, but also improves the social cohesion between immigrants and natives. Fostering integration of immigrants will help host societies to grow and prosper.

Gordon (1964) shows seven steps or stages for immigrants to integrate and assimilate. Integration can only be successful if all steps interplay with each other and are tackled by the immigrants. To those steps count for example the social interaction between immigrants and natives, like intermarriages. But also the legal framework has to be set in the host country to give the immigrants a chance to integrate.

One of those legal frameworks is granting citizenship of the host country. Literature shows that citizenship is highly correlated with better integration and occupational measures. There is evidence that naturalized immigrants earn more than non-naturalized ones (Chiswick, 1978; Steinhardt, 2012), have higher job-finding rates (Fougère and Safi, 2009; Gathmann and Keller, 2017) and experience steeper wage-tenure profiles (Bratsberg *et al.*, 2002). As the integration of children is especially important, many developed countries argue that it is important to integrate the children early on by granting birthright citizenship. Chapter 3 and 4 aim to show the effects of this.

But integration and assimilation cannot be achieved alone. Gordon (1964) also mentions two fundamental aspects of integration which have to be fulfilled by the host coun-

tries' society: being free from prejudice and discrimination. And here, again, public perception plays a crucial role.

1.4 Microeconometric Analysis

Microeconometric analysis is not insignificant when studying migration and integration. Correlational evidence might be biased, especially through the self-selection of people, into regions, into policy measures, into citizenship etc. Once a potential mechanism for causal inference is found, a second obstacle arises, finding fitting data. In what follows, I give more detailed information on exogenous variation and suitable data sets.

1.4.1 Causal Inference

When working on issues of migration and integration correlational evidence can lead to several issues. What if people move to a certain area because it is prosperous and with a low unemployment rate, or they move there because people are known to be the friendliest toward people from all over the world? What if people apply for citizenship of the host country, because they are perfectly integrated, know the language and their children thrive in the educational system? We would find that immigrants bring prosperity and high employment rates, we would find citizenship brings integration for both adults and children. But we would also know that there is no causal relation.

In those situations the best case solution would be to randomly distribute immigrants to regions. The amount of people, the academic background, innate abilities and origins of the immigrants should be distributed by chance. Also citizenships, language courses, and other integrative measures should also be randomly assigned among the people. Then, the correlational evidence would give us causal effects.

To approximate random assignment, scholars have found many possibilities to produce "as if" randomness. One of the most prominent examples is exogenous variations, for example due to unpredicted events, such as any sort of exogenous shock. One of these was used by Card (1990), using the example of the Mariel Boatlift, where 125,000 Cubans suddenly came to Florida. Another is the sudden flow of refugees during the Balkan wars, used for example by Angrist and Kugler (2003) and Speciale (2012). Another kind

of exogenous variation is the introduction of political reforms on a specific cut-off date. This allows us to compare individuals who are eligible for this political change and who are not eligible but are in fact only for example separated by birth by a couple of days.

In this thesis I use two different approaches for the causal inference. First, in Chapter 2, I use the outbreak of the Balkan Wars as an exogenous variation to instrument the percentage of immigrants in one region since, even though the outbreak of the civil war itself was exogenous, the settlement could not have been random.

The empirical analysis in Chapters 3 and 4 relies on a cut-off approach. I use the exogenous variation of the introduction of birthright citizenship as a random experiment for the granting of citizenship to second generation immigrant children. Chapter 4 combines this approach with an lab-in-the-field experiment with the same cohort.

1.4.2 Data

Once an interesting research idea and a setting are found where causal inference is possible, one of the major issues left is suitable data. The empirical analysis in this thesis uses a series of data sets. In what follows, I want to discuss the advantages and disadvantages of survey, administrative and experimental data and touch shortly upon the data sets used in this thesis.

First, I examine survey data which has several advantages. Most often survey data spans a large range of topics like family and individual background characteristics, it covers the opinions and attitudes of people and it gives detailed insights into personal circumstances, especially if done in a panel setting. However, it has also some disadvantages. Due to the large administrative costs, survey data is limited in sample size as every single observation is costly. Furthermore, it mostly just covers specific questions, which are regarded important in the specific setup of the survey. Most importantly it is always associated with the subjective answers of the individuals.

In this thesis I use several sets of survey data, especially in Chapter 2, which is completely based on survey data, and in Chapter 3, where survey data gives additional insights. The analysis in Chapter 2 is based mainly on the *Politbarometer*, a political survey conducted for German state TV, asking a representative sample of individuals who are allowed to vote. It gives a picture of public perception over a wide range of

political issues. Another data set used, which has also the advantage of a panel setup is the *German Socio-Economic Panel (GSOEP)*. This data set covers a wide range of topics and gives a large amount of control variables. Another panel used in Chapter 3 is the *National Educational Panel Study (NEPS)* . It gives a large range of information on children. All of the mentioned survey data sets have the disadvantage of having only small number of observations. The largest household survey in Europe is the German *Microcensus* with around 600,000 observations each year. It covers topics like family background, income, employment, education and many other interesting variables and is predominantly used in Chapters 2 and 3.

Second, administrative data covers the universe of the population, which solves the problem of achieving a representative sample of the population. Furthermore, it is based on official data records, which means subjective answers are not a problem. But most often analysis based on administrative data runs into one particular problem, namely the scarcity of information. Even though it most often gives a wide range of outcome information, it is much more limited as regards background information usable as control variables, since this is not crucial for administrative record-keeping.

The administrative data used in this thesis shows up especially in Chapter 3. Here we not only use school entry examination data, but also school record data, which covers the totality of children from one German federal state, Schleswig-Holstein. Both data sets cover a wide range of information on students, for example, their kindergarten attendance, examination data from doctors and school outcomes for children, such as school entry age, retentions and school tracking. Unfortunately, both data sets lack crucial background data, especially information on the parental background, such as the country of origin of the parents, education of the parents, or citizenship of the children and the parents.

Third, experimental data can combine advantages of both survey and administrative data. Incentivized experiments permit the behavior of individuals to be examined much more precisely than normal survey questions. Since experimental setups are mostly included in combination with an extensive background survey, the information provided is more informative than the administrative data. However, experimental data runs into the same problem as survey data. Every single observation is costly, even more so if the

experiments are incentivized. Furthermore, representativeness is crucial when setting up an experiment.

Chapter 4 is almost entirely based on our own collected data, which included an incentivized experiment that was combined with an extensive survey. In particular, we conducted a trust game as described by Berg *et al.* (1995) in 57 German schools in 2015. The study included over 4,000 students who completed the experiment and the survey. It gives us the advantage of combining outcomes of a trust game with natives and migrants, giving us an approximation of cooperative behavior between different groups in the population, with an extensive background and social survey data set.

1.5 Outline of the Thesis

This thesis spans three independent empirical studies. Each chapter gives its own contribution to the discussion on the public perception of immigrants and the integration of immigrant children.

Chapter 2 examines the public perception of immigrants during the Balkan Wars in Germany. It provides causal evidence on how the percentage of immigrants in a region affects the public concerns about immigrants. How society perceives events, like mass migration flows, is crucial on a political, economic, and social level. In the chapter, I examine to what extent the proportion of immigrants in one's region affects public opinion toward immigration. I focus on a specific time frame, the time during the Balkan Wars and look at Germany, to where about 700,000 refugees fled. I use an instrumental variables approach, where I use not only the exogenous timing of the Balkan Wars but also the exogenous variation in the distance of German cities to Sarajevo, assuming that the most important feature for migration into Germany is the distance to Sarajevo.

I find that the higher the percentage of immigrants in a region's population, the less the public is concerned about immigrants. Interestingly, the percentage of potential right-wing voting is increased. There are reasons to believe that this effect follows from peoples concerns about the economy and their own financial situation. European data depicts a similar situation for several European countries.

Chapters 3 and 4 are closely related to one another and deal with an important integrative measure, namely the introduction of birthright citizenship. Chapter 3 focuses on the integration effect on second-generation immigrant children and their early life education outcomes and spans the ages five to ten. Chapter 4 goes a step further and examines the same cohort of children in their last year of compulsory schooling and how their integration in non-market transactions evolved.

Chapter 3 is joint work with Christina Felfe and Helmut Rainer. This chapter examines whether the introduction of birthright citizenship in Germany had an impact on immigrant children's educational outcomes at the first three key stages of the education system: preschool, primary school, and secondary school. We employ a difference-in-differences strategy which exploits a birth date cut-off determining whether a child became eligible for birthright citizenship or not using a reform in the nationality law in Germany in the year 2000. We employ two administrative data sets for our analysis, school entry examination and school records of one German federal state, Schleswig-Holstein.

We find that the policy (i) increased immigrant children's participation in non-compulsory preschool education; (ii) had positive effects on key developmental outcomes (e.g. German language proficiency) measured at the end of the preschool period; (iii) caused immigrant children to progress faster through primary school; and (iv) increased the likelihood of them attending the academic track of secondary school, which paves the way for higher education.

Chapter 4 is joint work with Christina Felfe, Martin Kocher, Helmut Rainer, and Thomas Siedler. We focus on a fundamental aspect of migrant integration, namely non-market interactions between natives and immigrants. A central question in this context is how individuals of different, possibly oppositional, identities approach social dilemma situations.

We have (i) run an artefactual field experiment based on the trust game with a large sample (N=4,436) of German adolescents; (ii) allowed participants to condition their strategies on the migration background of their opponents; and (iii) matched the experimental data with individual background information from an extensive, self-conducted survey.

Our first result reveals an oppositional culture of trust and discrimination therein: native children extend their trust in almost equal measure to native and immigrant opponents, respectively. Immigrant children, by contrast, show a high degree of trust towards those with whom they share a migration background, but a low willingness to trust their native peers. On inspection, the differential trust decisions of immigrants turn out to be statistically unjustified and not driven by wrong stereotypes, which points to identity-based social preferences.

For our main contribution, we connect the artefactual field experiment with a natural policy experiment which saw the introduction of a widely debated integration policy: *birthright citizenship*, which automatically grants children born to foreign parents the nationality of the host country. We find that the policy substantially reduced the degree of trust discrimination among male, but not among female, immigrants. This effect is accompanied by an improved educational integration of immigrant males.

Chapter 2

Public Perception Toward Immigrants - Causal Evidence from Germany during the Balkan Wars

2.1 Introduction

Political and economic turmoil over the last three decades in East Europe, the Balkans, the Middle East, and Africa has had a substantial impact on global migration flows. The stock of foreign-born population in Western Countries is steadily increasing and is changing the face of the European countries. Since the end of the Cold War millions of migrants and refugees have relocated within and to Europe. These mass migration flows not only set many developed democracies on new demographic paths but also fueled public concerns about immigration and the integration of immigrants.

The public perception of immigration is key for societies on different political, social, and economic levels. First, flight and migration have changed the political arena in many Western democracies. Recent studies show that some of the most important current political decisions, such as UK's referendum to leave the European Union in 2016 and the 2016 U.S. presidential elections, where influenced substantially by the public debate over immigration (Klinkner, 2017; Harding, 2017). Pertinent intimidation by immigrant-related rhetoric can mobilize large segments of the electorate (Hainmueller *et al.*, 2015). Second, negative public perception leads to social tensions between native and immigrant groups, which contributes to social exclusion of immigrants (Dustmann and Preston, 2001). Third, negative public perception of immigration can have significant effects in terms of countries' prosperity and growth by influencing migration patterns and lessening business attractiveness (Siedler, 2011). The crucial underlying question behind public opinion toward immigration is whether individual concerns are due to a person's actual geographic proximity to immigrants or shaped by the mere salient presence of the topic across the country.

In this analysis, I examine whether the fraction of immigrants in the population has an effect on the natives' perception of immigrants and whether these perceptions have an impact on the political landscape. I explore this issue during the large influx of refugees into Germany during the course of the Balkan Wars in the early 1990s. I investigate whether the native population experiences immigrants as a pressing problem where the actual presence of immigrants is higher in their region. This is particularly interesting as it gives insights into the workings of a society and how the population

experiences and interprets certain events (Smith, 1980). To dig deeper into this issue and its potential consequences I investigate the impact on political attitudes and voting behavior on the fringes of society in particular, looking at right-wing political placement or voting behavior, which has a significant impact on the entire society. Finally, my interest focuses on potential channels of public perception, since those can be addressed or even changed by policy-makers. Such channels may be economic concerns or the fear of job loss due to higher competition from immigrants in the labor market. These channels can also be evaluated on a European level in order to gain insights into a broader spectrum of countries. Taken together, I draw a comprehensive picture of causes and consequences of public perceptions of immigrants.

To investigate the issue at hand, I look at a previous episode of a sudden immigrant inflow, that which occurred due to the Balkan Wars. The Balkan Wars at the beginning of the 1990s were the first wars on European soil since World War II. With the declaration of independence of Slovenia and Croatia in 1991 began one of the deadliest wars in that region. Two to three million people were either murdered or displaced during this time. It set off an unprecedented wave of refugee migration from the former Yugoslavian countries to Western Europe, only exceeded by the most recent refugee wave of 2015. At the end of the war in March 1995, 734,970 refugees were distributed across Europe, with the largest proportion migrating to Germany.

The migration flow from former Yugoslavia became an important part of the migration patterns in Europe in the early 1990s. This inflow is an interesting episode to analyze for several reasons. First, it is a quite sudden and unexpected rise in the inflow of immigrants. Second, it centers around a very specific group of immigrants, asylum seekers, which, in many contexts, should not foster the concerns of the native population. Third, the situation at that time resembles the situation faced in 2015 by Germany when about one million refugees from Syria and other countries fled there. The following analysis might, therefore, give some insights into the natives' reactions to such events.

Germany is an intriguing case for the study of public opinions of immigration. The country experienced by far the largest amount of immigrants from the former Yugoslavian countries. About half a million refugees fled to Germany in the year 1992 alone. Furthermore, since Germany had no well-established right-wing party in the early 1999s,

contrary to, for example, Austria or Denmark, voting for right-wing parties was especially small at the time; nonetheless, in times of crisis small right-wing parties can also mobilize a non-negligible part of the electorate[1]. Lastly, Germany has recently experienced a similar refugee inflow which makes conclusions from previous episodes even more important. To establish external validity on a European level, I also extend my analysis to several European countries at the end of my investigation.

Several data sets – administrative and survey – allow me to answer the question of whether the proportion of immigrants in a region has an effect on the public opinion toward immigrants. I focus on the distribution of the immigrant population across 30 West German districts in the years around the outbreak of the Balkan Wars, 1990 to 1992. For the dependent variables, I use data from Politbarometer, which includes information gathered monthly on general attitudes towards politically relevant issues. The Politbarometer also asks the question as to what is the most pressing problem in Germany at the moment. The answers to this question give insights into the workings of a society and how the population experiences and interprets events (Smith, 1980). I construct the dependent variable by setting the variable to one if refugees or immigrants are viewed as the most pressing problem for Germany at the moment, otherwise zero. If these perceptions also translate into political preferences, I can analyze using data on political placement and potential voting behavior, especially when looking at the political fringe of a society. In particular, I investigate extreme-right political placement and extreme-right voting. Using the German Socio-economic Panel (GSOEP) I am able to look into potential channels, for example, economic worries, such as fear of job loss or general worries about the economic situation. Furthermore, I use data from the statistical offices of the German federal states on the percentage of immigrants in different regions, specifically register data from the Office of Foreigners that gives an exact percentage of foreigners in each region. To create external validity, I investigate the channels on a European level. I use data from the Eurobarometer and the OECD for eleven states in the years 1987 to 1995.

[1]In the early 1990s, a party called the *Republikaner* gained momentum during the migration inflows, as did the party Alternative for Germany during the refugee crisis in 2015.

There are no clear theoretical predictions regarding the effect of the proportion of immigrants on natives' concerns about immigrants. On the one hand, Campbell (1965) suggests that prejudice arises from the conflict of interests between groups. Competition for scarce resources can aggravate hostile behavior toward other groups. Competition can arise over employment possibilities or over limited resources in the social system. An important feature of this theory is that even the perception of such competition for resources can increase intergroup conflicts. It is not necessarily the actual competition that matters.

On the other hand, Allport (1954) in his contact theory describes that more direct contact with either immigrants or refugees could decrease reservations towards immigrants. This transformation in beliefs is due to better experiences through the contact with immigrants than prejudices or third-party information would have predicted. Contact on the one hand and competition for scarce resources on the other are closely entangled and add up to its distributional consequences. The competition between natives and immigrants for jobs or government spending and services is a fundamental aspect. In such situations, LeVine and Campbell (1972) expect a negative turn to the contact theory, where proximity could even lead to increasing hostility. According to the theory, it might be possible to find positive perceptions of immigration in the proximity of immigrants (Allport, 1954), but also negative reactions from the fraction of the population which perceives competition for scarce resources (Esses *et al.*, 1998).

Correlational evidence shows that the higher the proportion of immigrants in a region, the more concerned the population is about immigration, as it views immigrants and asylum seekers as the most pressing problem facing Germany. These findings would suggest that the proportion of immigrants during the Balkan Wars indeed has had a negative effect on natives' perception of immigrants.

To examine the causal effect of the percentage of immigrants in one region on the public perception of natives of immigrants, I use an exogenous variation. To prevent any potential bias or reverse causality, I rely on an instrumental variables (IV) approach using the distance to Sarajevo during the Balkan Wars as an IV for the proportion of

immigrants in West German regions[2]. This approach offers the possibility of analyzing how attitudes change based on the decisions of immigrants to migrate to regions close to their home country during the war. The mere distance right after the outbreak of the Balkan Wars can be assumed to be exogenous to preexisting attitudes of natives towards immigrants and their economic and political concerns.

Using this IV approach to estimate a causal effect between the proportion of foreigners and natives' perception of immigrants, I find that a one percent increase in immigrants leads to a five percentage points (ppt) decrease in natives' concerns about asylum seekers, and an eight ppt decrease towards immigrants in general. This amounts to a ten percent change in the mean. Whereas OLS estimates find an increase in concerns about immigrants, the IV approach shows a substantial decrease in the natives' apprehensiveness in regions with a high percentage of immigrants. When examining the political fringe of society, the results are different, as I find an increase of potential right-wing voting behavior, which is in line with the findings from previous literature. Furthermore, the higher proportion of immigrants lead to a higher increase in worries about the economy and one's own financial situation. This result stems mainly from lower educated persons and persons with low incomes. Evidence from Europe underline the results from Germany. Several robustness checks confirm the results.

What is missing so far in the literature is, on the one hand, the combination of the two literature strands; as Hainmueller and Hopkins (2014) point out, so far the literature on attitudes toward immigration and the effects of immigration on the political arena have developed in isolation. This study is an attempt to combine these two strands. On the other hand, causal evidence in contrast to correlational evidence on the proximity of immigrants on the public perception of immigration is scarce. To establish such causal evidence is another contribution of this analysis to the literature. In this chapter, I take the first step to shedding light on the public perception of immigrants by asking (i) how is the public perception of immigrants different across the country in the light of a massive inflow of immigrants and (ii) how does this perception interplay with individual

[2]A similar approach for European countries was used by Angrist and Kugler (2003) and Speciale (2012).

backgrounds and potential channels. In what follows, I highlight the contribution of the paper to the related literature.

I contribute to the literature in several ways. First, I contribute to the strand of literature on attitudes towards migrants, but in doing so, use causal interference from an exogenous inflow shock. Second, like Halla *et al.* (ming), I look at how natives' voting behavior changes when the migrant population increases. Because Germany, unlike Austria, did not have a well-established right-wing party at that time, I instead look at self-placement on the political spectrum, and also provide evidence for very small right-wing parties. I enhance the analysis using an exogenous shock and, by also including personal attitudes, I can identify the channels for different political placements. Third, I am not looking only at migrants in general, but at the inflow of asylum seekers, a group of people who are looking for refuge in the host country. That is, they do not have economic reasons for migrating, and thus should not be any threat to existing job opportunities. Refugees do not compete for jobs with the native population but might very well be perceived as a competition for financial resources.

There is an extensive literature studying the effects of immigration on the attitudes of natives, from both a within-country and cross-national perspectives (Card *et al.*, 2012; Hainmueller and Hiscox, 2007). Across OECD countries there is heterogeneity in the impact of immigrant populations on the perception natives have of them. In countries where those perceptions seem rather negative, the negative attitudes stem from a lower level of education (Scheve and Slaughter, 2001; Hainmueller and Hiscox, 2010) and personal characteristics (Steinhardt and Poutvaara, 2015). Additional factors for negative perceptions are perceived higher crime rates (Fitzgerald *et al.*, 2012) and there are ambiguous results for the fear of losing jobs (Mayda, 2006; Hainmueller *et al.*, 2015). The literature on such attitudes deals with the question of how natives' attitudes are affected by the presence of immigrants. Dustmann and Preston (2007), Pedersen *et al.* (2006) and O'Rourke and Sinnott (2006) study the effects of negative sentiments toward immigrants.

More recent studies focus on the effects of immigrants on the voting behavior of natives and find quite ambiguous results. Beginning with Otto and Steinhardt (2014), who find positive impacts of immigrants on right-wing parties, several other papers deal with the relationship between immigrants and the rise of right-wing parties. Halla *et al.*

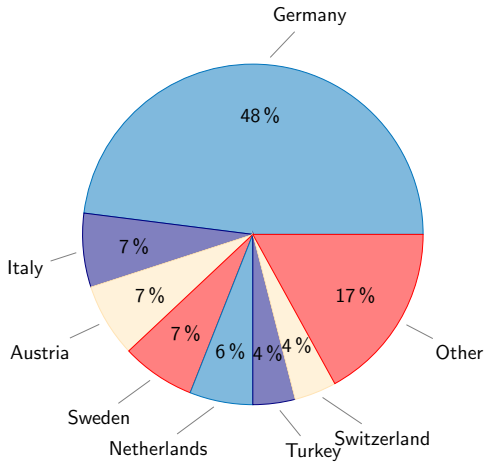
(ming) and Gerdes and Wadensjö (2008) look at how natives' voting behavior is affected by the number of immigrants in their neighborhoods and conclude that an increased percentage of immigrants in one's region is related to higher voter turnouts for right-wing parties. Furthermore, Barone *et al.* (2016) and Brunner and Kuhn (2014) look at Italy and Switzerland. In contrast, Steinmayr (2016) finds negative effects on voter turnout for right-wing parties when looking at the most recent episode of refugee inflow in Austria. On a similar topic, Geishecker and Siedler (2011) look at how the fear of job loss can foster right-wing voting.

The remainder of the chapter is structured as follows. Section 2.2 describes the background and the different data sets used for my analysis are discussed in Section 2.3. Section 2.4 introduces the empirical strategy. Section 2.5 discusses the used identification strategy at length. Section 2.6 presents the main results and provides a series of robustness checks and heterogeneous effects. Section 2.7 provides the conclusion.

2.2 The Balkan Wars and the Public Perception in Germany

With the fall of the Iron Curtain, many countries in East Europe experienced violent structural processes, especially in former Yugoslavia. The unstable economic and political situation of the 1980s motivated an independence movement that gained momentum throughout the former Yugoslavian region, culminating in June 1991 in the declaration of independence by Slovenia and Croatia, which in turn, led to Europe's deadliest conflict since World War II. Between August and September 1991, 80,000 Croats were displaced from the Croatian regions where Serbs were in power. At the end of 1991, Germany acknowledged Slovenia and Croatia as independent states; other EU countries then followed suit. At the beginning of 1992, a referendum on independence was held in Bosnia. This led to a siege of the city Sarajevo, which culminated in the terrible massacre of Srebrenica in 1995, in which 8,000 Moslems were killed. In December 1995, a peace agreement was reached and the war finally ended. Two to three million people became victims of the war, as they fled, were expelled, or murdered. Almost half of the refugees went to Germany (see Figure 2.1), causing a tremendous inflow of asylum seekers in the early 1990s.

Figure 2.1: Asylum seekers from former Yugoslavia by host country



Notes: These data stems from estimations by the UNHCR in March 1995. At that point in time around 734,970 refugees were distributed around Europe.

Germany, which until then had no experience in large immigrant inflows (except for the guest worker program of the 1950s and 1960s[3]), was the largest recipient of asylum seekers from the Balkan Wars in the early 1990s. UNHCR reported in March 1995 that Germany was the host country for almost half of the refugees from this civil war (see Figure 2.1).

Figure 2.2 gives us an overview of the applications of asylum seekers in the years between 1990 and 2015 in Germany, where we can see two main peaks. One of them was in 1992 during the Balkan Wars, during the time period assessed by this analysis, and another occurred in recent years with its peak in 2015. Bureaucrats and courts were swamped with asylum inquiries. The increase in asylum inquiries led, on the one hand, to extended stays of asylum seekers in Germany, as they were allowed to stay until their inquiry was processed and, on the other, to occasional violent resentment on the part of the native population.

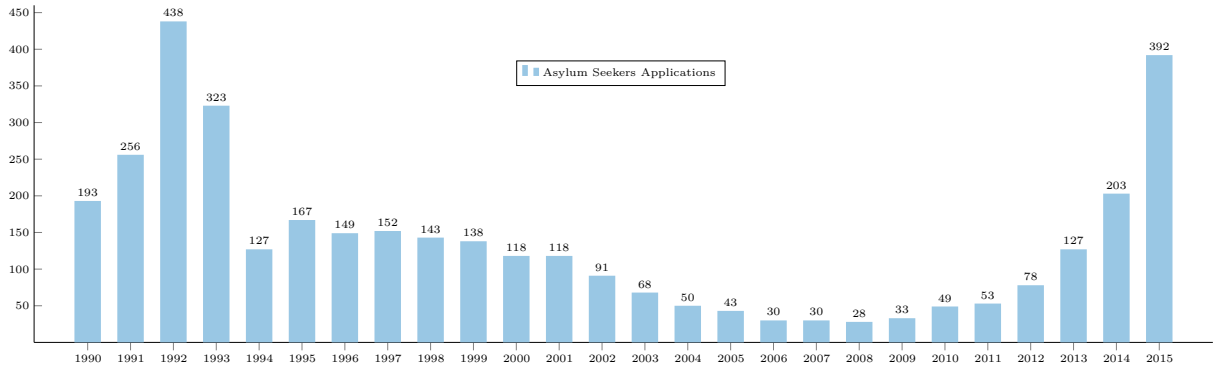
Following an extreme peak of asylum seekers in 1992, Germany ratified the asylum compromise (*Asylkompromiss*) in 1993, setting strict rules for asylum seekers and their inquiries, while procedures were speeded up. EASY (*Erstverteilung der Asylsuchenden* = a system to first distribute asylum seekers around the German regions) was introduced and asylum seekers had to stay in their assigned region. Asylum seekers from countries which were perceived as safe by the German federal government or asylum seekers who first passed through so-called safe countries were not permitted to apply for asylum status in Germany.

These new measures substantially decreased the number of asylum seekers in Germany. The allocation of immigrants, however, was not enforced by any sanctions. Even if immigrants were allocated to one region, they would shortly thereafter move to another region. Some municipalities were overwhelmed by immigrants which led to an

[3]From 1955 onward Germany started a so-called guest worker program to satisfy the immense need for immigrant labor in order to rebuild Germany. The first countries sending immigrant labor were Italy (1955), Spain and Greece (1960), with other countries like Turkey (1961), Portugal (1964) and Yugoslavia (1968) following suit. Recruited immigrant workers were called guest workers (Gastarbeiter). The name already implied that the stay of the workers were expected to be only temporary (Algan *et al.*, 2012). Hence, the willingness of immigrants to integrate and thus effective integration policies were completely lacking. Overall 14 million immigrants came to Germany during the years of the guest worker program, with around 11 million people leaving the country again. In the early 1970s, the immigrant population constituted 4.9% of the German population. In the wake of the first oil crisis in 1973 the guest worker program was canceled due to the economic decline which made the hiring of foreign workers obsolete.

official complaint by the municipalities in 1995. From this followed an allocation procedure in 1996, which sanctioned immigrants by cutting off any financial help if they moved to another region.

Figure 2.2: Applications of asylum seekers in Germany (in thousands)



Source: BAMF (Bundesamt für Migration und Flüchtlinge)

The public perception of immigrants and in particular refugees and the media coverage were quite ambiguous at that time. On the one hand, it was acknowledged that the Balkan Wars and the subsequent migration flows constituted the most serious humanitarian crisis since World War II and that help for refugees was essential. On the other hand, public concerns were fueled by how Germany would be able to handle the inflow of so many people, and more essentially how all immigrants and refugees could be integrated. At that time Germany also experienced an uprising of far-right political movements and criminal behavior against refugees, but also immigrants in general.

Comparing media coverage between the years 1992 and 2015 shows some similarities in the documentation of the refugee crisis during the Balkan Wars and the refugee crisis due to the Syrian civil war. These similarities cover the necessity for humanitarian help, the hostility of some parts of the population against immigrants and the blaming of the European Union for not dealing with the problem. This highlights the importance of the topic even 25 years later.

2.3 Data

In order to answer the research questions empirically and to establish a relationship between the share of immigrants in a region to the attitudes of natives, I require compre-

hensive data. First, I use population data from all the regions in my sample to calculate the immigrant shares for every region (see the first line in Table 2.1). Furthermore, I use immigrant inflow data from the OECD over this period to measure immigrants in one country. These are all 30 West German regions[4], which are smaller than a federal state, but larger than counties. The population data is gathered from the statistical offices of the federal states.

Second, I draw upon two social surveys in Germany, the *Politbarometer* and the German Socio-Economic Panel. Both datasets are available at regional level and for the given time frame. Third, for evidence on the European level, I infer from data from the *Eurobarometer* and data from the OECD. In what follows, I describe the three data sets in more detail.

2.3.1 Politbarometer

The *Politbarometer* is a monthly survey conducted from the *Forschungsgruppe Wahlen* (election research group) for German state TV. The survey targets the voting population in Germany and asks about parties, politicians and the most pressing issues in society. I restrict the years of the analysis to one year before the outbreak of the war, 1990, and two years afterward, 1991 and 1992, and the months of September to December since the outbreak of the Balkan War occurred in September. The restrictions leave us with a sample size of 11,939 individuals. The *Politbarometer* asks the following question:

What is in your opinion the most important problem the federal republic is facing at the moment?

Even though this question gives more details on the relative ranking of problems rather than the absolute measurement of public anxieties, it provides insights into the workings of society and how the population perceives certain events (Smith, 1980).

The questionnaire offers several possible answers. This closed question approach has the advantage of describing changes in public opinion over time and differences among different regions (Schuman and Scott, 1987) As the main objective of this analysis is public opinion toward migration, I construct the following binary variables:

[4]Regierungsbezirke in German

- *Immigrants as a Problem:* equals one if asylum seekers or foreigners are the most pressing or second most pressing problem Germany is facing, zero otherwise

- *Asylum Seekers as a Problem:* equals one if asylum seekers are the most pressing or second most pressing problem Germany is facing, zero otherwise

- *Foreigner as a Problem:* equals one if foreigners are the most pressing or second most pressing problem Germany is facing, zero otherwise

Table 2.1 shows, on the one hand, an increase of 1 percentage point (ppt) of the foreign population in Germany before and after the outbreak of the Balkan Wars and, on the other, a substantial increase before and after the outbreak of the civil war in terms of public opinion toward immigration. The increase of the variable "Immigrants as a Problem" of over 50 percentage points (ppt) is especially noticeable. This increase is mainly driven by the variable "Asylum Seekers as a Problem" (an increase of 40 ppt), but also the variable "Foreigners as a Problem" sees a quite substantial increase (14.3 ppt).

The literature suggests that the presence of foreigners may also have an effect on the political attitudes of natives. This is of particular interest as in Hainmueller *et al.* (2015) it was pointed out that so far there is no connection between public attitudes and the political process. It is important when looking at that fringe of society which might be mobilized by anti-immigrant rhetoric. This is especially true for far-right extremism. To evaluate this, I use survey data from the *Politbarometer* to construct the following binary variables:

- *Extreme-right Placement:* equals one if the person places him/herself on the most extreme right of the political scale from 1 to 10, otherwise zero.

- *Extreme-right Voting:* captures potential extreme-right voting behavior (Republikaner or NPD[5]) if elections were to be held on the Sunday following the interview.

Even though there is no substantial shift in the extreme-right political placement, there is a positive shift in the extreme-right voting of 2.5 ppt. Although this is survey

[5]For a detailed description of the extreme-right political landscape in Germany in the early 1990s see Otto and Steinhardt (2014).

Table 2.1: Descriptive statistics of outcome variables before and after the outbreak of the Balkan Wars - Politbarometer

	Mean	Before	After	Difference
Foreigners	0.087	0.080	0.090	0.010***
Public Opinion Toward Immigrants				
Foreigners as Problem	0.143	0.047	0.190	0.143***
Asylum Seekers as Problem	0.337	0.069	0.47	0.401***
Immigrants as Problem	0.468	0.114	0.643	0.529***
Voting Behavior				
Extreme-right Placement	0.062	0.058	0.063	0.005
Extreme-right Voting	0.025	0.008	0.033	0.025***

Notes: *Before* refers to the year before the outbreak of the civil war on the Balkan and it is restricted to the months September-December. *After* refers to the years after the outbreak, 1991 and 1992 and also to the months September-December. The variables *Foreigner as a Problem* and *Asylum Seekers as a Problem* stem from the *Politbarometer* and are set to 1 if the respondent named either of them as the first or second most prominent problem in Germany. The variable *Immigrants as a Problem* combines both of the previous variables and is set to 1 if either of them is equal to 1. *Extreme-right Placement* stems also from the *Politbarometer* and is set to 1 if the respondent set its own political placement to 1 on a ten-step scale. *Extreme-right Voting*, also generated from the *Politbarometer* is set to 1 if the respondent would vote either *Republikaner* or *NPD* if on Sunday elections were held.

data and not actual voting behavior and might therefore show a biased picture, observed differences still give insights in the political landscape at the time. If we look at the election outcomes from the year 1994, we see that the Republikaner won 1.9 % of the electorate, which is similar to the survey results. The largest results were achieved in the federal states Bavaria, Baden-Württemberg, and Hessia, the federal states in the south of Germany, closest to the former Yugoslavian countries. Table A.1 in the Appendix shows the individual covariates before and after the outbreak of the Balkan War, which do not change over time.

2.3.2 German Socio-Economic Panel

Anti-migrant sentiments could stem from a variety of reasons. The most prominent argument is the economic well-being of citizens, notably in the context of the perceived competition for scarce resources, such as jobs or governmental spending. The *German Socio-economic Panel (GSOEP)* gives me the opportunity to further investigate natives' public opinions, especially regarding their potential channels which lead to negative perceptions of immigrants. As interviews were conducted mainly at the beginning of each year, I restrict my sample to individuals interviewed between the months January to June in the years 1991 to 1993. This gives me a sample of 17,529 individuals. The GSOEP allows me to construct the following binary variables:

- *Job Insecurity:* equals one if individuals are worried about losing their jobs, zero otherwise
- *Worry about the Economic Situation:* equals one if individuals are worried about the economy in general, zero otherwise
- *Worry about one's own Financial Situation:* equals one if individuals are worried about their own financial situation, zero otherwise

Concerns over scarce resources seem also to have increased in this time period. "Worry about the Economic Situation" in general increases by 13 ppt., "Worry about one's own Economic Situation" increases by 2.4 ppt., and the "Job Insecurity" increases by 1.9 ppt. Table A.2 in the Appendix not only shows that, except for those under 25 years of age,

population characteristics do not change over time, but also that the samples of the two data sets, *Politbarometer* and *GSOEP*, are similar.

Table 2.2: Descriptive statistics of outcome variables before and after the outbreak of the Balkan Wars - GSOEP

	Mean	Before	After	Difference
Channels				
Worry about the Economic Situation	0.272	0.187	0.317	0.13***
Worry about one's own Financial Situation	0.109	0.094	0.117	0.024***
Job Insecurity	0.056	0.043	0.062	0.019***

Notes: *Before* refers to the year before the outbreak of the civil war on the Balkan and it is restricted to the months January to June 1991. *After* refers to the years after the outbreak, 1992 and 1993 and also to the months January to June. *Worry about the Economic Situation, Worry about one's own Economic Situaion and Job Insecurity* are all also generated from the GSOEP and are set to 1 if individuals are very worried about the individual subjects.

2.3.3 Eurobarometer and OECD Data

Other countries in Europe have also received substantial amounts of immigrants. To check whether the findings from Germany are also applicable to a broader set of countries, I use data from the Eurobarometer. I use data from eleven European countries: Belgium, UK, Italy, France, Germany, Denmark, Norway, Portugal, Spain, the Netherlands, and Luxembourg. Since I can draw only on such a small sample of countries, I have extended the time horizon and analyzed the years between 1987 and 1995. In addition, I am only using data from surveys conducted in the fall, normally between September and November, in order to use only those surveys conducted during wartime.

These restrictions lead to a sample of 167,789 observations. There are no questions regarding immigrants or asylum seekers in these datasets at that time period, but I am able to look at some variables related to politics and economics. Specifically, I analyze the effect of the geographic proximity of immigrants on the right-wing political placement, the attitudes towards the economy as a whole and on the financial situation of the household of the native population:

Table 2.3: Descriptive statistics of outcome variables before and after the outbreak of the Balkan Wars - Eurobarometer

	Mean	Before	After	Difference
Inflow of Foreigners	165,562	150,517	176,362	25,845***
Extreme-right Placement	0.054	0.06	0.05	-0.01***
Worry about the Economic Situation	0.441	0.365	0.494	0.13***
Worry about one's own Financial Situation	0.281	0.26	0.296	0.036***

Notes: *Before* refers to the year before the outbreak of the civil war on the Balkan and it is restricted to September to December 1987-1990. *After* refers to the years after the outbreak, 1991-1995 and also to the months September to December. *Worry about the Economic Situation, Worry about one's own Economic Situaion and Job Insecurity* are all also generated from the Eurobarometer and are set to 1 if individuals are very worried on the individual subjects. Information on the *Inflow of Foreigners* stems from OECD data.

- *Extreme-right Placement:* equals one if the person places him/herself on the most extreme right of a scale of 1 to 10, otherwise zero.
- *Worry about the Economic Situation:* equals one if individuals are worried about the economy in general, zero otherwise
- *Worry about one's own Financial Situation:* equals one if individuals are worried about their own financial situation, zero otherwise

Table 2.3 presents descriptive statistics for the variables before and during the Balkan Wars. One can observe a substantial increase in the worries about the economic situation and a smaller increase in the worry over one's own financial situation in the European countries. The extreme-right political placement even decreases in Europe. Furthermore, I use immigrant inflow data from the OECD over this period to measure immigrants in one country. Table 2.3 displays that over the years the countries experienced an average inflow of foreigners of 165,562. The largest recipient were Germany and the UK.

2.4 Empirical Approach

This section introduces first the equation of interest and describes why OLS regression might lead to biased results. In the next step, I present a fixed effects framework and its potential caveats before lastly turn to the IV identification strategy used in this analysis.

2.4.1 OLS Regression

The purpose of this chapter is to evaluate the effect of the geographic proximity of immigrants on the public perception of immigration of the native population. In order to estimate a causal effect of the immigrants on the public perception of immigration one would like to calculate the difference in public opinion toward immigration from individuals living in one region without any immigrant with an individual living in one region with the presence of immigrants. Ideally, if one lives in a region with and without immigrants would be decided randomly.

Obviously, in a globalized world it is almost impossible to find regions without any immigrants presence. Therefore, I investigate the effect of the intensity of the presence of immigrants on the public opinion of immigration. To establish a relationship between the share of immigrants in one's region and the public perception of native inhabitants the ordinary least square equation appears as follows:

$$Y_i = \alpha + \beta ln(s_{st}) + \gamma X_i + \epsilon_{ist} \tag{2.1}$$

where Y_i is an individual perception and $ln(s_{st})$ the log of immigrant percentages in region s in year t. Measuring the share of immigrants in the population instead of the labor force is important as natives in regions with high shares of immigrants might live next to them and experience immigrants in their everyday lives and thus form their opinions of immigrants, but might not work with them.

When just looking at the correlation between the percentage of immigrants in the region and natives' perception toward immigration is not clear in which direction the causality is working. What we would like to see is that immigrants come into a region and because of this the public changes its perception toward immigrants, in a positive or negative way, which theoretically is not clear. But it could also be that because of either

positive or negative view toward immigration migration patterns are shaped. In order to tackle this problem, I am introducing regional fixed effects, which I present in the next subsection.

2.4.2 Fixed Effects Regression

Region and time fixed effects are introduced in order to control for unobserved but time-invariant omitted variables, like a general negative attitudes toward immigration due to previous immigration flows into the region. In particular, the settlement of workers from the former Yugoslavian countries at the end of the *guestworker programme* were relatively stable since 1974 until the outbreak of the Balkan Wars. Introducing region fixed effects, control for this settlement pattern. It is important to control for observed confounding factors. If omitted variables are fixed on a regional level, it is possible to control for them by implementing region dummies (Angrist and Pischke, 2008).

$$Y_i = \alpha + \beta ln(s_{st}) + \gamma X_i + \tau WarYears_t + \psi Region_s + \epsilon_{ist} \tag{2.2}$$

where

$WarYears_t$ is a binary variable which indicates the years 1991 and 1992

$Region_s$ are 30 region fixed effects

There are however reasons to believe that these results are biased. For example, improved labor market conditions can attract immigrants and improve attitudes toward immigrants, as they are needed to bolster the workforce. Changing labor market conditions might not be completely captured by observed variables like unemployment rate or income. In this instance, OLS estimates would be biased downward. An economic downturn can increase the immigrant inflow due to lower housing prices, but can also negatively influence attitudes toward immigrants as people lose their jobs and need somebody to blame, a scenario that would push FE estimates upward.To be able to deal with these potential biases, I am furthermore using an IV for the percentages of immigrants in order to get causal estimates, which I explain and discuss in the following subsections.

2.4.3 Instrumental Variables Strategy

As seen in Figure 2.2, Germany experienced a sharp increase of Yugoslavs among its immigrants, suggesting that the distance to Sarajevo during the Balkan Wars may be a good predictor for the share of foreigners in the early 1990s. Furthermore, Figure 2.3 shows the percentage change in foreign shares between 1991 and 1992, the peak of the immigrant inflow. Even though it only shows raw inflow data, a tendency is observable that regions in the south experienced a higher inflow.

As set out in the previous subsection, OLS regressions on the effect on natives' attitudes of the percentage of immigrants in a region might be biased. To avoid any biased result and to be able to look at causal inference, I instrument the percentage of immigrants by the distance to Sarajevo, using the Balkan Wars as an exogenous shock. The first stage estimate appears as follows:

$$ln(s_{st}) = \tau WarYears_t + \psi Region_s + b_{st}\pi_s + \eta_{ist} \tag{2.3}$$

where

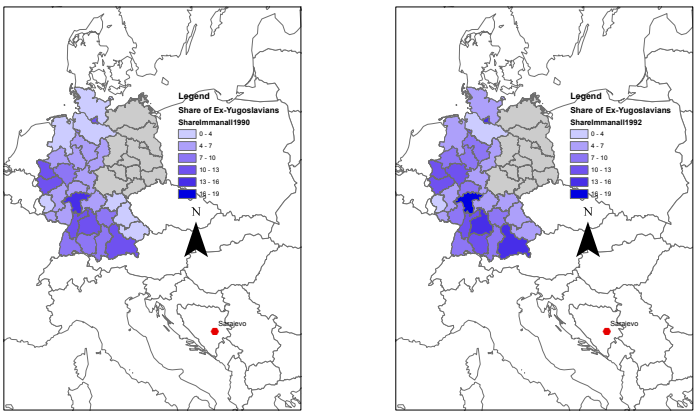
$WarYears_t$ controls for a war years t,

$Region_s$ includes region dummies s,

and b_{st} is the distance from Sarajevo $* \; WarYears_t = 1991/1992$.

Figure 2.3: Inflow of immigrants during the Balkan Wars

(a) Share 1990 (b) Share 1992



Source: Foreign Statistics 1990-1992, own calculations

I compute the distance to Sarajevo by taking the distance from Sarajevo to the capital of each region by using data from *Google Maps*. The distances vary between 992 km from Sarajevo to Munich in the south of Germany, and 1820 km from Sarajevo to Kiel in the north[6].

I now proceed with the predicted values $\widehat{ln(s_{st})}$ to estimate the second stage of the IV approach (see equation 2.4). The resulting estimate β corresponds to the local average treatment effect (LATE).

$$Y_i = \alpha + \beta\widehat{ln(s_{st})} + \tau WarYears_t + \psi Region_s + \gamma X_i + \varepsilon_{ist} \tag{2.4}$$

where

$WarYears_t$ controls for a war years t,

$Region_s$ includes region dummies s,

and X_{it} controls for individual background characteristics

Exclusion restriction requires that the instrument must not be correlated with the error term in the structural equation. In this context, the exclusion restriction requires

[6]Angrist and Kugler (2003) and Speciale (2012) used a similar strategy for Europe, which I will use to underline my results also on the European level.

that the mere distance between Sarajevo during the Balkan Wars and any German region is not correlated with the public perception of immigrants over time, other than by changing the percentage of immigrants in this country. The main concern that in general the distance to Sarajevo could be correlated with natives' attitudes, as the distance is correlated with other attitude-forming characteristics, can be ruled out. I discuss threats to identification in the following section. The validity of the instrument is presented in the next section.

2.5 The Instrument

In this section I contrast the results from using the instrument on a regional and an individual level, I discuss the distance measure and give examples for different possibilities of measuring the distance. Lastly, I discuss why other potential distributional mechanisms do not confound the instrument.

Before looking at the results of this study, it is crucial to have an in-depth evaluation of the instrument that I am using to verify the connection between the percentage of immigrants in a region and possible effects on the attitudes of natives. As is shown in the Table 2.4, the instrument is highly significant on a 1% level. The point estimate of -0.012 is equivalent to a 1.2 ppt decrease of immigrants in a population by a 1,000 km distance. Bearing in mind that the difference between the cities closest to and furthest from Sarajevo in Germany is about 900 km, the estimate comes close to the effect of the inflow of asylum seekers during the 1990s.

Data set — Using different data sets could also lead to the possibility that the different data set composition will lead to different first stage results. As seen in Table 2.4, the estimate for the effect of the distance during the Balkan Wars is the same for both data sets, 1,000 km difference amounts of a decrease of 1.2 ppt.

Distance measures — A further critique is whether the distance measure using *Google maps* is biased since it is measured with the present street infrastructure, which might not resemble the infrastructure of the early 1990s. To address this issue, I use an alternative measure for distance, calculating the direct distance to the calibrated center

Table 2.4: First-stage results - different samples

	Politbarometer	GSOEP
Distance*war years	-0.012***	-0.011***
	(0.003)	(0.003)
Observations	11,939	17,529
War Dummy	yes	yes
Region Dummies	yes	yes

Notes: Robust standard errors are reported in parentheses. *** (**) (*) indicates significance at the 1% (5%) (10%) level.

of the district. Table 2.5 displays the difference of these two distance measures. The mean of the direct distance is around 400 km shorter than the distance calculated with street infrastructure.

Table 2.5: Alternative distance measures

	Mean	Minimum	Maximum
Sarajevo-Capital in km	1,408.2	992	1,820
Sarajevo-Centroid in km	1,033.41	690,75	1,306.21
Zagreb-City in min.	786	506	1038

Notes: The variable Google-City is calculated with the online service *Google maps* and shows the kilometers of the fastest way to the main city of the district. Sarajevo-Centroid is the calculated direct distance to the calibrated center of the district. Zagreb-City in min. displays train rides in minutes from Zagreb to the region.

Reviewing the media outlets of that time shows that many asylum seekers came by train to Germany[7]. Distances covered by train or by car can vary quite substantially, in particular because Germany opened its first high-speed train track in 1991. I explore whether it makes a difference if I use distances by car or by train by using a rich dataset of train timetables from the year 1991. As asylum seekers were transported out from

[7]A phenomenon which was also observed during the refugee crisis in 2015.

the war zone from Zagreb, I am calculating the train rides in minutes from Zagreb to the cities in the region, which had either access to the ICE (high-speed train) or were fastest to reach. Table 2.5 shows the average minutes, 786, which corresponds to about 13 hours. Again, the closest city is Munich with 8.5 hours and the most remote city is Kiel with around 17 hours.

Furthermore, the effect of the distance might not be linear. Non-linearity arises when, for example, with every kilometer of distance the costs for per kilometer decrease. I use the log of the distance measure for a potential non-linear effect to address this issue.

Table 2.6: First-stage results - alternative distance measures

	Sarajevo-Centroid	Zagreb-City in 100 min.	Log(distance)
Distance*war years	-0.025***	-0.001**	-0.021***
	(0.002)	(0.000)	(0.005)
Observations	11,939	11,939	11,939
War Year Dummy	yes	yes	yes
Regional Dummies	yes	yes	yes

Notes: Robust standard errors are reported in parentheses. *** (**) (*) indicates significance at the 1% (5%) (10%) level.

Table 2.6 shows that the sign of the coefficient of 1,000 km distance to Sarajevo difference does not change by using an alternative measure. The coefficient doubles, however, as it does when using the log of distance. This means that there is also not much change using a non-linear effect. The effect of the distance for train rides seems rather weak, as the immigrant share changes by 0.1 percent for every 100 min train ride.

Timing — For the main analysis, I use a dummy variable for the two war years, 1991 and 1992, to control for variation in time. To increase accuracy I introduce year dummies in Table 2.7. This has no significant effect on the first stage. Furthermore, I introduce a time trend instead of a wartime dummy variable, which also does not change the first stage estimates.

Table 2.7: First-stage results - different time controls

	Time Dummies	Time Trend
Distance*war years	-0.011***	-0.010***
	(0.002)	(0.000)
Observations	11,939	11,939
Time Dummies	yes	no
Time Trend	no	yes
Regional Dummies	yes	yes

Notes: Robust standard errors are reported in parentheses. *** (**) (*) indicates significance at the 1% (5%) (10%) level.

What else is related — Distance can also be related to a lot of things. As the region dummies should control for many regional variations, I would like to relate the instrument to several individual characteristics. Table 2.8 shows the result for characteristics such as income, employment, primary education, and religion. Neither the coefficients nor the F-statistics are significant.

Table 2.8: What else could be related with distance?

	Income	Employment	Low Education	Catholic
Distance*war years	-4.081	-0.006	-0.012	0.044
	(18.193)	(0.043)	(0.043)	(0.337)
F-statistics	5.95	2.25	7.16	121.56
Observations	17,529	11,939	11,939	11,939
War Year Dummy	yes	yes	yes	yes
Regional Dummies	yes	yes	yes	yes

Notes: Robust standard errors are reported in parentheses. *** (**) (*) indicates significance at the 1% (5%) (10%) level.

Königssteiner Schlüssel— In contrast to former studies like Angrist and Kugler (2003) and Speciale (2012), the main identification strategy in this project is used by instrumenting the percentage of foreigners in just one country, Germany, instead of all European countries. A valid concern in this context is that Germany at that time already had a distributional mechanism in place, the so-called Königssteiner Schlüssel. Under this distributional mechanism, asylum seekers were first taken to the nearest home for asylum seekers, where they applied for official asylum status. These registered asylum seekers were then supposed to be distributed across the country following a mechanism calculated on the basis of population and tax revenues.[8]

Evaluating the data at hand reveals that Baden-Württemberg, a German federal state in the south, experienced an increase of around 50,000 persons from the former Yugoslavian countries between 1991 and 1992, which is around 20% of all Yugoslavs coming to Germany that year. Schleswig-Holstein, in contrast, experienced an increase of 1630, or 2.4%. If we compare that to the quotas set by the Königssteiner Schlüssel, Baden-Württemberg received over 7 ppt more people from the former Yugoslavian countries than it was supposed to, and Schleswig-Holstein, 1 ppt fewer.

The Königssteiner Schlüssel was first and foremost a mechanism for distributing asylum seekers across federal states. In order to control for this distribution over federal states, I do not use region dummies when calculating the first stage, but I introduce federal state dummies while using regional clusters. The different estimation strategy leads to a different coefficient, namely -0.430 (see table 2.9), which is still significant. When controlling for a variable displaying the percentage distribution of the Königssteiner Schlüssel, the variable distance*war years is not impacted. Also controlling for the population size and tax revenues in 100,000 has no impact. Again, neither of these elements has a confounding effect on the effect of the distance to Sarajevo during the Balkan Wars. Even calculating the real distributional mechanism by using 1/3 population and 2/3 tax income it has no effect.

A further evidence for the plausibility of the instrument stems from the results of the first stage in the years 1992-1994. After the *Asylkompromiss* in 1993, distribution

[8]More precisely, distribution was calculated taking into account tax revenues weighted by two-thirds and population weighted by one-third.

Table 2.9: Distance to Sarajevo vs. Königssteiner Schlüssel

	1990-92			1992-94		
Distance*war years	-0.430*	-0.480**	-0.648**	-0.151	-0.195	-0.244
	(0.225)	(0.235)	(0.304)	(0.342)	(0.350)	(0.365)
KS		-0.570*			-0.533+	
		(0.311)			(0.347)	
Population			-0.006**			-0.004*
			(0.003)			(0.002)
Tax Revenues			0.415*			0.243*
			(0.218)			(0.150)
Observations	90	90	90	90	90	90
War Year Dummy	yes	yes	yes	yes	yes	yes
Federal State Dummies	yes	yes	yes	yes	yes	yes

Notes: Data comes from the federal statistical offices. This table shows several variations of the first stage, controlling for potential effects of the Königssteiner Schlüssel, or an effect of an asylum agreement which was undertaken in the light of high inflows of asylum seekers. Standard errors are clustered on a region level. *** (**) (*) indicates significance at the 1% (5%) (10%) level.

mechanisms were enforced more severely and the number of asylum seekers in those years declined. By looking at the right-hand part of the table, it is possible to observe that the coefficient of interest, *Distance*war years*, is now much smaller and not significant anymore, which means that the previous assumption that with every km from Sarajevo the percentage of immigrants in the population is no longer valid. This leads to the conclusion that the negative effect of the distance to Sarajevo on the percentage of immigrants in the years between 1991 and 1992 correlated to the distance to Sarajevo.

2.6 Results

This section presents first the differences between OLS regression results, fixed effects regression results and the causal evidence from the IV strategy. It proceeds with a sensitivity analysis, where I look at (i) different answers to what are the most pressing problems of the country, (ii) previous settlement structure, (iii) reduced form estimates

with and without a t-wild bootstrap cluster. Next, I investigate the fringe of society by looking at right-wing political placement and voting behavior. Furthermore, I investigate potential channels for the findings, which might be connected to perceived competition for scarce resources, such as fear of job loss, and worries over the economy and one's own financial situation. I also examine at the outcomes for different groups of the population. The section concludes with checking the external validity of the results by using European data for eleven countries during the years 1987 and 1995.

2.6.1 Correlation vs. Causal Evidence

Looking at simple OLS estimates, we see that an increased proportion of immigrants in a region leads to a higher probability that foreigners are seen as a problem by the population. In particular, a one percent increase of foreigners increases the probability that immigrants are seen as a problem by 0.107 percentage points (ppt). Introducing individual controls like age, gender, education, employment status, and denomination has no effect on the result. This small positive effect is even more emphasized when introducing region and war fixed effects. In Table 2.10 we can see that a one percent increase in the share of foreigners increases the probability of concerns over immigrants by 1.265 ppt. in the fixed-effect model.

Table 2.10 shows a different picture of the causal effect of the percentage of immigrants on the perception that immigrants are seen as a problem, that is, the IV results. It shows that, in comparison to the OLS and the FE estimates, it does not have a positive effect but rather a substantial negative effect on the probability that individuals perceive immigrants as a problem, namely -8.055 ppt. This results suggests a 12.5% decrease in the mean. The F-statistic is also reasonable large to exclude a weak instrument. This means that the more immigrants in a region, the fewer individuals see them as a problem. This is in line with the contact theory by Allport (1954), where natives who are in contact with migrants experience the proximity to migrants as more positive than people who are not in such close contact.

Once the variable is split into Asylum Seekers as a Problem and Foreigners as a Problem it is observable that the main driver of the effect are the asylum seekers seen as a problem, as a one percent increase in foreigners leads to a 1.189 ppt. increase in the

Table 2.10: Results on the public perception of immigrants

	Immigrants					
	OLS	FE	IV	OLS	FE	IV
ln(% of Foreigners)	0.107***	1.265***	-8.185**	0.112***	1.263	-8.055**
	(0.010)	(0.101)	(3.758)	(0.010)	(0.101)	(3.713)
Observations	11,939	11,939	11,939	11,935	11,935	11,935
F-statistics			13.10		13.22	
Distance*war years			-0.012***		-0.012***	
			(0.003)		(0.003)	
War Year Dummy	no	yes	yes	no	yes	yes
Region Dummies	no	yes	yes	no	yes	yes
Individual Controls	no	no	no	yes	yes	yes

Notes: The variable *Immigrants as a Problem* stems from the *Politbarometer* and are set to 1 if the respondent named either of them as the first or second most prominent problem in Germany. The variable *Immigrants as a Problem* combines both of the previous variables and is set to 1 if either of them is equal to 1. All estimates include region dummies and a time trend. The mean of the variable during the Balkan Wars is reported in []. Individual controls include gender, age, education, civil status, employment status and denomination. Distance is measured with *Google maps* and shows the kilometers from Sarajevo to the main city of the district. Robust standard errors are reported in parentheses. *** (**) (*) indicates significance at the 1% (5%) (10%) level.

perception that asylum seekers are one of the main problems in Germany. Also, in this case, we can see that the driver of the effect is clearly asylum seekers (see Table 2.11). They are seen as less of a problem by -5.610 ppt., which is an eighth of the overall increase during the Balkan Wars. Foreigners, in general, do not seem to have a significant effect on negative perceptions.

2.6.2 Sensitivity Analysis

Major problem — So far, the dependent variables were coded as 1 if immigrants (asylum seekers or foreigners) were the most or the second most pressing problem in

Table 2.11: Results on the public perception of asylum seekers or foreigners

	Asylum Seekers			Foreigners		
	OLS	FE	IV	OLS	FE	IV
ln(% of Foreigners)	0.084***	1.188***	-5.526*	0.026***	0.150*	-2.607
	(0.009)	(0.105)	(3.136)	(0.007)	(0.084)	(2.228)
Observations	11,935	11,935	11,935	11,935	11,935	11,935
F-statistics			13.22			13.22
Distance*war years			-0.012***			-0.012***
			(0.003)			(0.003)
War Time Dummy	no	yes	yes	no	yes	yes
Region Dummies	no	yes	yes	no	yes	yes
Individual Controls	yes	yes	yes	yes	yes	yes

Notes: The variables *Foreigner as a Problem* and *Asylum Seekers as a Problem* stem from the *Politbarometer* and are set to 1 if the respondent named either of them as the first or second most prominent problem in Germany. The variable *Immigrants as a Problem* combines both of the previous variables and is set to 1 if either of them is equal to 1. All estimates include region dummies and a time trend. Individual controls include gender, age, education, civil status, employment status and denomination. Distance is measured with *Google maps* and shows the kilometers from Sarajevo to the main city of the district. Robust standard errors are reported in parentheses. *** (**) (*) indicates significance at the 1% (5%) (10%) level.

Germany. To check whether the effect comes not from the second most pressing problem, but that it is really in the mind of the population, I show also the results for just the most pressing problem. Table A.4 in the Appendix presents the descriptive statistics.

Table 2.12 displays the 2nd stage results for the most pressing problem in Germany. The coefficient for *Immigrants as a Problem* is slightly smaller than in 2.10, so is also the variable for asylum seekers. This result indicates that the effect is not driven by the second most pressing problem but rather by the most prominent.

Other problems — The results on the public perception of immigration so far are based on the answer to what the most pressing problem in the country is. It is possible that, in the regions where more immigrants are residing, other problems dominate, but

Table 2.12: Immigrants as major problem in Germany

	Immigrants	Asylum Seekers	Foreigners
	IV	IV	IV
ln(% of Foreigners)	-7.462**	-5.440*	-2.021
	(3.407)	(2.820)	(1.824)
Observations	11,935	11,935	11,935
F-statistics	13.22	13.22	13.22
Distance*war years	-0.012***	-0.012***	-0.012***
	(0.003)	(0.003)	(0.003)
War Time Dummy	yes	yes	yes
Region Dummies	yes	yes	yes
Individual Controls	yes	yes	yes

Notes: The variables *Immigrants as a Problem*, *Foreigner as a Problem* and *Asylum Seekers as a Problem* stem from the *Politbarometer* and are set to 1 if the respondent named either of them as the first or second most prominent problem in Germany. The variable *Immigrants as a Problem* combines both of the previous variables and is set to 1 if either of them is equal to 1. All estimates include region dummies and a time trend. Individual controls include gender, age, education, civil status, employment status and denomination. Distance is measured with *Google maps* and shows the kilometers from Sarajevo to the main city of the district. Robust standard errors are reported in parentheses. *** (**) (*) indicates significance at the 1% (5%) (10%) level.

Table 2.13: Results on other potential problems

	Right-wing Extremism	Ethnic Germans	Social Problems	Environment	Reunification
ln(% of Foreigners)	-1.713	0.951	2.264	0.595	-1.885
	(1.242)	(1.129)	(3.258)	(2.907)	(3.391)
Observations	11,935	11,935	11,935	11,935	11,935
F-statistics	13.22	13.22	13.22	13.22	13.22
Distance*war years	-0.012***	-0.012***	-0.012***	-0.012***	-0.012***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
War Year Dummy	yes	yes	yes	yes	yes
Region Dummies	yes	yes	yes	yes	yes
Individual Controls	yes	yes	yes	yes	yes

Notes: The variables *Right-wing Extremism as a Problem*, *Ethnic Germans as a Problem* , *Social Problems*, *Environment as a Problem*, and *Reunification as a Problem* stem from the *Politbarometer* and are set to 1 if the respondent named either of them as the first or second most prominent problem in Germany. The variable *Immigrants as a Problem* combines both of the previous variables and is set to 1 if either of them is equal to 1. All estimates include region dummies and a time trend. Individual controls include gender, age, education, civil status, employment status and denomination. Distance is measured with *Google maps* and shows the kilometers from Sarajevo to the main city of the district. Robust standard errors are reported in parentheses. *** (**) (*) indicates significance at the 1% (5%) (10%) level.

are essentially related to the immigrants. This could be housing, or job availability, which I have summarized into *Social Problems*. Other arguments, as for example the reunification of Germany, or the environment, cannot be directly connected to the percentage of immigrants in a region. If another problem, connected to immigrants or otherwise, could be explained by the identification strategy, it would show that the results are biased.

Table 2.13 shows the coefficients for several alternative answers. *Right Extremism* is the problem arising from extreme right-wing violence and parties. *Ethnic Germans* relates to ethnic Germans from Eastern European countries as the most pressing problem. *Social Problems* are connected to job insecurity, housing, pensions etc. *Environment* is a binary variable set to one if the environment is the most important problem in Germany. *Reunification* is the problem related to unification and its financing. Descriptive statistics

how these problems have changed before and during the Balkan Wars are displayed in Table A.5. Neither of the coefficients shows a significant effect, which means that (i) in regions with more immigrants the negative effects stem not from other problems overlapping the concerns about immigrants and (ii) these problems are not affected by the percentage of immigrants in one region

Ethnic enclaves— After leaving the war zone, migrants might not have gone to the nearest region, but to a region where friends and maybe family members were already residing. Moving to relatives means that the percentage of foreigners in a region during the early 1990s was not driven by the distance to Sarajevo, but by the settlement structure of former migrants. This could violate the exclusion restriction of the IV and thus bias the results because attitudes of natives might be already different in regions where we find higher percentages of foreigners already before the war started. But account should be taken of the regional fixed effects., as already described in Section 2.4.2.

Reduced Form — Another concern is the clustering of the standard errors. An obvious level of clustering would be at the regional level. Since in the analysis there are only 30 regions, it would be too small a cluster size to calculate reliable standard errors. I deal with this issue by first presenting the reduced form in Table 2.14 and then comparing these results with a t-wild cluster bootstrap approach[9] .

The reduced form equation appears as follows:

$$Y_i = \tau WarYears_t + \psi Region_s + b_{st}\pi_s + \eta_{ist} \tag{2.5}$$

where

$WarYears_t$ controls for a war years t,

$Region_s$ includes region dummies s,

and b_{st} is the distance from Sarajevo $* WarYears_t = 1991/1992$.

The coefficient of the reduced form estimate can be interpreted as follows: 1000 km distance from Sarajevo during the Balkan Wars increases the negative perception

[9]I estimate the wild cluster bootstrap standard errors using 1000 replications under H_1 as discussed in Cameron *et al.* (2008).

of immigrants by 9.7 ppt. Hence, the reverse conclusion indicates that living closer to Sarajevo and therefore experiencing more immigrants in one's region leads to a decrease of negative concerns, which is a decrease in the mean of around 15%.

Table 2.14: Reduced form results on public perception of immigrants, asylum seekers and foreigners

	Immigrants		Asylum Seekers		Foreigners	
	RF	RF t-wild cluster	RF	RF t-wild cluster	RF	RF t-wild cluster
Distance*war years	0.097***	0.097	0.067**	0.067	0.029	0.029
	(0.034)	[0.02]	(0.031)	[0.15]	(0.025)	[0.30]
Observations	11,935	11,935	11,935	11,935	11,935	11,935
War Year Dummy	yes	yes	yes	yes	yes	yes
Region Dummies	yes	yes	yes	yes	yes	yes
Individual Controls	yes	yes	yes	yes	yes	yes

Notes: The variables *Foreigner as a Problem* and *Asylum Seekers as a Problem* stem from the *Politbarometer* and are set to 1 if the respondent named either of them as the first or second most prominent problem in Germany. The variable *Immigrants as a Problem* combines both of the previous variables and is set to 1 if either of them is equal to 1. All estimates include region dummies and year dummies. Robust standard errors and t-wild cluster bootstrap p-values are reported in parentheses. Individual controls include gender, age, education, civil status, employment status and denomination. Distance is measured with *Google maps* and shows the kilometers from Sarajevo to the main city of the district. *** (**) (*) indicates significance at the 1% (5%) (10%) level.

2.6.3 Immigrants and Politics

A high percentage of foreigners in a region has a negative effect on the increase in seeing either immigrants or asylum seekers as a problem, a result that is quite contrary to most of the literature so far, which states that a higher percentage of immigrants is also related to an increase in extreme-right voting. The interesting question is now whether this more positive view of immigrants will also relate to political interests. To evaluate this, I look at three different, related variables. First, if individuals change their own political placement, second, would they be more or less likely to vote for extreme-right parties and third, are they even interested in politics?

Table 2.15: Results on extreme-right political placement and voting

	Extreme-right Placement		Extreme-right Voting	
	FE	IV	FE	IV
ln(% of Foreigners)	-0.013	2.357	0.307***	2.487**
	(0.054)	(1.806)	(0.040)	(1.217)
Observations	11,935	11,935	11,935	11,935
F-statistics		13.22		13.22
Distance*war year		-0.012***		-0.012***
		(0.003)		(0.003)
War Year Dummy	yes	yes	yes	yes
Region Dummies	yes	yes	yes	yes
Individual Controls	yes	yes	yes	yes

Notes: *Extreme-right Placement* stems also from the *Politbarometer* and is set to 1 if the respondent set its own political placement to 1 on a ten-step scale. *Extreme-right Voting*, also generated from the *Politbarometer* is set to 1 if the respondent would vote either *Republikaner* or *NPD* if on Sunday elections were held. *Political Interest* is generated from the GSOEP and is set to 1 if the respondent is very interested in politics. All estimates include region dummy variables and a wartime dummy variable. Individual controls include gender, age, education, civil status, employment status and denomination. Distance is measured with *Google maps* and shows the kilometers from Sarajevo to the main city of the district. Robust standard errors are reported in parentheses. *** (**) (*) indicates significance at the 1% (5%) (10%) level.

There is no significant effect on a higher extreme-right political placement. A higher percentage of immigrants actually led to a higher percentage of those potentially voting for extreme-right parties, namely by 2.487 ppt., which is actually the mean difference between before and after the beginning of the war. This finding is actually in line with the previous literature. The interest in politics seems not to be changed.

2.6.4 Immigrants and Economic Worries

If public concerns toward immigrants do not increase, but the probability of voting for extreme-right parties does, the question is why? One explanation could be that natives fear immigrants as they fear job losses due to the higher supply of labor. For this

analysis, I use only natives who are actually employed, as unemployed persons are not likely to fear job loss. The results in the first sample point at first to increased job insecurity. Controlling for individual characteristics reveals that an increased percentage of foreigners actually has a negative correlation on job insecurity. An increase of the immigrant inflow decreases the fear of job loss by 0.861 ppt, which means that worrying about job loss cannot be an explanatory factor for increased right-wing voting.

Table 2.16: Results on job and economic insecurity

	Job Insecurity		One's Own Economic Situation		Economic Situation	
	FE	IV	FE	IV	FE	IV
ln(% of Foreigners)	-0.027	0.861	0.091+	3.893*	1.155***	5.403*
	(0.050)	(1.588)	(0.061)	(2.144)	(0.087)	(2.830)
Observations	9,379	9,397	16,559	16,559	16,601	16,601
F-statistics		12.37		16.27		16.86
Distance*war years		-0.013***		-0.012***		-0.012***
		(0.004)		(0.003)		(0.003)
War Year Dummy	yes	yes	yes	yes	yes	yes
Region Dummies	yes	yes	yes	yes	yes	yes
Individual Controls	yes	yes	yes	yes	yes	yes

Notes: *Worry about the Economic Situation, Worry about one's own Economic Situation and Job Insecurity* are all also generated from the GSOEP and are set to 1 if individuals are very worried on the individual subjects. All estimates include region dummy variables and a wartime dummy variable. Individual controls include gender, age, education, civil status, employment status and denomination. Distance is measured with *Google maps* and shows the kilometers from Sarajevo to the main city of the district. Robust standard errors are reported in parentheses. *** (**) (*) indicates significance at the 1% (5%) (10%) level.

What can, however, explain extreme-right voting is the effect of the percentage of immigrants on the worry about the individual economic situation and the worry about the overall economic situation. This finding is also in line with previous literature, as it is not job insecurity that is the main driver of extreme-right voting in the light of a high percentage of immigrants, but individual economic well-being.

Table 2.17: Reduced form results on public perception of immigrants - heterogeneities

	Low Educ		High Educ		Employed	
	RF	RF t-wild cluster	RF	RF t-wild cluster	RF	RF t-wild cluster
Distance*war years	0.139***	0.139	0.032	0.032	0.082*	0.082
	(0.050)	[0.06]	(0.069)	[0.63]	(0.044)	[0.10]
Observations	5,778	5,778	2,681	2,681	7,062	7,062
War Year Dummy	yes	yes	yes	yes	yes	yes
Region Dummies	yes	yes	yes	yes	yes	yes
Individual Controls	yes	yes	yes	yes	yes	yes

Notes: The variables *Immigrants as a Problem* stems from the *Politbarometer* and are set to 1 if the respondent named either of them as the first or second most prominent problem in Germany. The variable *Immigrants as a Problem* combines both of the previous variables and is set to 1 if either of them is equal to 1. All estimates include region dummies and year dummies. Individual controls include gender, age, education, civil status, employment status and denomination. Distance is measured with *Google maps* and shows the kilometers from Sarajevo to the main city of the district. Robust standard errors and t-wild cluster bootstrap p-value are reported in parentheses. *** (**) (*) indicates significance at the 1% (5%) (10%) level.

2.6.5 Heterogeneities

The important question is what are the characteristics of some of the behavior seen. We know from the extensive literature that certain characteristics are related to attitudes toward immigration. Such characteristics are education, income, sex, age, and also bitterness. Because of this I also analyze the perception of immigrants by social categories, namely low and high education and employment. Table 2.17 shows that a low education level and people who are employed that are the driving factors making the difference between regions with more or fewer immigrants. The same holds for right-wing voting, as seen in the Table 2.18.

When examining which part of societies are more worried over the economy or their own financial situation, I use the GSOEP data and stratify the sample again, by low and high educated individuals, the employed, but also individuals who were earning an equivalent of under 500 € net. The effect on the worry about the economy stems mainly

Table 2.18: Reduced form results on extreme-right voting - heterogeneities

	Low Educ		High Educ		Employed	
	RF	RF t-wild cluster	RF	RF t-wild cluster	RF	RF t-wild cluster
Distance*war years	-0.067***	-0.067	0.001	0.001	-0.030*	-0.030
	(0.023)	[0.07]	(0.013)	[0.95]	(0.018)	[0.08]
Observations	5,778	5,778	2,681	2,681	7,062	7,062
War Year Dummy	yes	yes	yes	yes	yes	yes
Region Dummies	yes	yes	yes	yes	yes	yes
Individual Controls	yes	yes	yes	yes	yes	yes

Notes: The variables *Foreigner as a Problem* and *Asylum Seekers as a Problem* stem from the *Politbarometer* and are set to 1 if the respondent named either of them as the first or second most prominent problem in Germany. The variable *Immigrants as a Problem* combines both of the previous variables and is set to 1 if either of them is equal to 1. All estimates include region dummies and year dummies. Individual controls include gender, age, education, civil status, employment status and denomination. Distance is measured with *Google maps* and shows the kilometers from Sarajevo to the main city of the district. Robust standard errors and t-wild cluster bootstrap p-value are reported in parenthesis. *** (**) (*) indicates significance at the 1% (5%) (10%) level.

from lower educated individuals. Not surprisingly, individuals earning under 500 € net are among those especially worried about their own financial situation, but so are both low and high educated individuals.

Table 2.19: Reduced form results on worries over the economic situation - heterogeneities

	Low Educ		High Educ		Employed		Under 500 €	
	RF	RF t-wild cluster	RF	RF t-wild cluster	RF	RF t-wild cluster	RF	RF t-wild cluster
Distance*war years	-0.105*	-0.105	0.096	0.096	-0.068*	-0.068	-0.057	-0.057
	(0.061)	[0.16]	(0.066)	[0.08]	(0.039)	[0.10]	(0.043)	[0.25]
Observations	4,354	4,354	3,088	3,088	9,751	9,751	8,562	8,562
War Year Dummy	yes	yes	yes	yes	yes	yes	yes	yes
Region Dummies	yes	yes	yes	yes	yes	yes	yes	yes
Individual Controls	yes	yes	yes	yes	yes	yes	yes	yes

Notes: The variable *Worry about the Economic Situation* stem from the *GSOEP* and are set to 1 if the respondent named either of them as the first or second most prominent problem in Germany. All estimates include region dummy variable and a wartime dummy variable. Individual controls include gender, age, education, civil status, employment status and denomination. Distance is measured with *Google maps* and shows the kilometers from Sarajevo to the main city of the district. Robust standard errors and t-wild cluster bootstrap p-values are reported in parentheses. *** (**) (*) indicates significance at the 1% (5%) (10%) level.

Table 2.20: Reduced form results on worries over one's own financial situation - heterogeneities

	Low Educ		High Educ		Employed		Under 500€	
	RF	RF t-wild cluster	RF	RF t-wild cluster	RF	RF t-wild cluster	RF	RF t-wild cluster
Distance*war years	-0.083*	-0.083	-0.074*	-0.074	-0.039	-0.039	-0.052*	-0.052
	(0.049)	[0.25]	(0.044)	[0.08]	(0.028)	[0.22]	(0.031)	[0.14]
Observations	4,338	4,338	3,082	3,082	9,742	9,742	8,525	8,525
War Year Dummy	yes	yes	yes	yes	yes	yes	yes	yes
Region Dummies	yes	yes	yes	yes	yes	yes	yes	yes
Individual Controls	yes	yes	yes	yes	yes	yes	yes	yes

Notes: The variables *Foreigner as a Problem* and *Asylum Seekers as a Problem* stem from the *Politbarometer* and are set to 1 if the respondent named either of them as the first or second most prominent problem in Germany. The variable *Immigrants as a Problem* combines both of the previous variables and is set to 1 if either of them is equal to 1. All estimates include region dummies and year dummies. Individual controls include gender, age, education, civil status, employment status and denomination. Distance is measured with *Google maps* and shows the kilometers from Sarajevo to the main city of the district. Robust standard errors and t-wild cluster bootstrap are reported in parentheses. *** (**) (*) indicates significance at the 1% (5%) (10%) level.

2.6.6 Public Perception in Europe

Germany is an interesting country for which to study immigration, as it has turned out to be one of the most attractive European countries for immigrants in the last few decades. Not only did it experience the highest inflow of immigrants during the Balkan Wars, but also in the recent immigration crisis of 2015. Still, it was not the only country to experience high immigrant inflows in those periods.

Table 2.21: European public perceptions

	Extreme-Right Placement		Economic Situation		One's Own Economic Situation	
	FE	IV	FE	IV	FE	IV
Inflow foreigners in 100,000	-0.001	-0.003	0.054***	0.100***	0.012***	0.008
	(0.000)	(0.007)	(0.003)	(0.021)	(0.003)	(0.019)
Observations	84,167	84,167	53,135	53,135	53,135	53,135
F-statistics		4,353		1,879		1,879
Distance*war years		-0.559***		-0.434***		-0.435***
		(0.008)		(0.010)		(0.010)
War Year Dummy	yes	yes	yes	yes	yes	yes
Region Dummies	yes	yes	yes	yes	yes	yes
Individual Controls	yes	yes	yes	yes	yes	yes

Notes: All the variables are coming from the Eurobarometer between 1987 and 1995. *Extreme-Right Placement* is set to 1 if individuals place themselves on a 9 or 10 on a ten-point-scale of political placement between left and right. *Economic Situation and One's Own Economic Situation* are set to 1 if individuals have the feeling that the economy in general or one's own economic situation in the household are worse than last year. All estimates include country dummies and a time trend. Individual controls include gender, age, education, civil status, employment status and denomination. Robust standard errors are reported in parentheses. Distance is measured with *Google maps* and shows the kilometers from Sarajevo to the main city of the country. *** (**) (*) indicates significance at the 1% (5%) (10%) level.

Table 2.21 shows that the first stage of the IV approach is also highly significant in the European context. A 1,000 km distance to Sarajevo decreases immigrant inflow by 55,900. I set the war year dummy equal to one in the years between 1991 and 1995.

The results of the second stage in the European sample point in the direction of the results for Germany. It also seems that, in the European context, the population saw no change in the extreme right-wing political placement. Additionally, there is evidence of an increased insecurity regarding the performance of the economy as a whole and of one's personal financial situation. The fact that this result can also be found in the European context is striking.

2.7 Conclusion

In the last decades, migration to Western countries has increased substantially. Migration has become a salient topic in the public debate in many developed democracies. The public concerns about immigration have an impact on societies on several levels, political, economic and social. The causal effect of the actual geographic proximity of immigrants on individuals' perception of immigration, however, has barely been studied.

This study explores how the proportion of foreigners in one region can affect the attitudes of natives in that region toward immigrants. This analysis also investigates the exact type of immigrants toward which this concerns are directed, asylum seekers. Identification is based on the Balkan Wars of the early 1990s. Drawing on two social surveys – Politbarometer and GSOEP – allows me to employ an IV approach. I provide evidence that the larger the proportion of immigrants in a region, the weaker the public concerns toward immigrants, especially towards asylum seekers. Nonetheless, the geographic proximity to immigrants increases extreme-right voting at the fringe of the society. It additionally shows that a larger proportion of foreigners in a region leads to more concern about the economic situation in general and one's individual economic situation in particular among the natives. This evidence is also verified in the European context, using data of the Eurobarometer for the years 1987 to 1995.

This analysis gives not only causal evidence of the effect of geographic proximity to immigrants on public perceptions but also combines two strands of literature, attitudes toward immigrants and the effect of immigration on the political arena. Furthermore, I provide evidence on the public perception of immigration in the light of a mass inflow

of asylum seekers and thus, might give further insights in the recent inflow of asylum seekers in 2015 and the public perception of this event.

Appendix A

Table A.1: Descriptive statistics of individual covariate variables before and after the outbreak of the Balkan Wars - Politbarometer

	Mean	Before	After	Difference
Individual Covariates				
Male	0.48	0.475	0.482	0.007
Under 25 years old	0.105	0.111	0.102	-0.009
Over 60 years old	0.124	0.121	0.125	0.004
Hauptschule	0.484	0.477	0.488	0.011
Realschule	0.283	0.295	0.277	-0.018**
Abitur	0.225	0.220	0.227	0.007
Married	0.008	0.009	0.007	-0.001
Employed	0.592	0.590	0.593	0.003
Catholic	0.426	0.429	0.424	-0.004
Protestant	0.445	0.451	0.442	-0.009

Notes: *Before* refers to the year before the outbreak of the civil war on the Balkan and it is restricted to the months September-December. *After* refers to the years after the outbreak, 1991 and 1992 and also to the months September-December.

Table A.2: Descriptive statistics of individual covariate variables before and after the outbreak of the Balkan Wars - GSOEP

	Mean	Before	After	Difference
Individual Covariates				
Male	0.477	0.478	0.476	-0.002
Under 25 years old	0.139	0.155	0.130	-0.024***
Over 65 years old	0.155	0.150	0.157	0.007
Elementary Education	0.263	0.262	0.263	0.000
Secondary Education	0.546	0.549	0.544	-0.005
Tertiary Education	0.184	0.184	0.184	0.000)
Married	0.665	0.659	0.668	0.009
Employed	0.587	0.595	0.584	-0.011

Notes: *Before* refers to the year before the outbreak of the civil war on the Balkan and it is restricted to the months January to June. *After* refers to the years after the outbreak, 1992 and 1993 and also to the months January to June.

Table A.3: Descriptive statistics of characteristics variables before and after the outbreak of the Balkan Wars - Eurobarometer

	Mean	Before	After	Difference
Male	0.484	0.478	0.488	0.009***
Under 25	0.146	0.154	0.139	-0.015***
Over 65	0.145	0.141	0.148	0.007***
Low Education	0.283	0.301	0.268	-0.033***
Intermediate Education	0.374	0.362	0.385	0.023***
High Education	0.343	0.337	0.347	0.01***
Married	0.647	0.652	0.642	-0.0***
Employed	0.737	0.755	0.723	-0.032***
Catholic	0.448	0.461	0.44	-0.021***
Protestant	0.175	0.167	0.18	0.014***

Notes: *Before* refers to the year before the outbreak of the civil war on the Balkan and it is restricted fall surveys before 1991. *After* refers to the years after the outbreak, after 1991 until 1995.

Table A.4: Descriptive statistics of outcome variables before and after the outbreak of the Balkan Wars - major problem

	Mean	Before	After	Difference
Public Perception of Immigrants				
Foreigner as a Major Problem	0.104	0 .023	0.144	0.121***
Asylum Seekers as a Major Problem	0.25	0.032	0.358	0.326***
Immigrants as a Major Problem	0.354	0.055	0.502	0.448***

NOTES: *Before* refers to the year before the outbreak of the civil war on the Balkan and it is restricted to the months September-December. *After* refers to the years after the outbreak, 1991 and 1992 and also to the months September-December. The variables *Foreigner as a Major Problem* and *Asylum Seekers as a Major Problem* stem from the *Politbarometer* and are set to 1 if the respondent named either of them as the first most prominent problem in Germany. The variable *Immigrants as a Major Problem* combines both of the previous variables and is set to 1 if either of them is equal to 1.

Table A.5: Descriptive statistics of outcome variables before and after the outbreak of the Balkan Wars - other problems

	Mean	Before	After	Difference
Other Problems				
Right-wing Extremism as a Problem	0.052	0.002	0.077	0.075***
Ethnic Germans as a Problem	0.021	0.035	0.014	-0.021***
Social Problems	0.266	0.307	0.246	-0.061***
Environment as a Problem	0.155	0.251	0.108	-0.143***
Reunification as a Problem	0.362	0.564	0.262	-0.302***

NOTES: *Before* refers to the year before the outbreak of the civil war on the Balkan and it is restricted to the months September-December. *After* refers to the years after the outbreak, 1991 and 1992 and also to the months September-December. The variables *Right-wing Extremism as a Problem* and *Ethnic Germans as a Problem*, *Social Problems*, *Environment as a Problem*, and *Reunification as a Problem* stem from the *Politbarometer* and are set to 1 if the respondent named either of them as the first most prominent problem in Germany.

Chapter 3

Why Birthright Citizenship Matters for Immigrant Children: Short- and Long-Run Impacts on Educational Integration*

This chapter is joint work with Christina Felfe from the University of St. Gallen and Helmut Rainer from the ifo Institute and the University of Munich.

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3.1 Introduction

Children with migrational backgrounds constitute the fastest-growing segment of the population in many countries across the developed world, shaping these societies for the future (Dustmann *et al.*, 2012; Tienda and Haskins, 2008). Immigrant families bring many strengths to their host countries, but they also present serious policy challenges. A core concern, hotly debated by scholars and policymakers alike, is the educational performance of immigrant children. Although there are some countries where certain groups of immigrant children do educationally at least as well as children of the native-born, there are many others where they are, on average, outperformed by their native peers in school. In Europe, for example, a good deal of evidence points to a substantial immigrant disadvantage in multiple indicators of academic achievement.[10] Not surprisingly, tackling this disadvantage has become a key priority for many governments. Yet, relatively little is known about which interventions could be effective in fostering immigrant children's educational integration.

In this project, we examine one important instrument of integration policy: immigrant children's access to the host-country nationality. Specifically, we exploit a natural experiment in Germany which saw the introduction of conditional *birthright citizenship*. Through the reform, a significant portion of immigrant children automatically acquired German nationality by birth, and thus the same legal rights – and as such political and professional opportunities – as their native counterparts. From this, as we will argue in more detail below, it can be hypothesized that the reform increased the returns on education for immigrants, created incentives for human capital investments, and thus fostered immigrant children's educational integration.

To assess the empirical relevance of this hypothesis, we take a dynamic view of the education process and analyze the impact of birthright citizenship on a series of educational indicators measured at the first three stages of the German education system: preschool, primary school and secondary school. Some specific questions we address include: did the introduction of birthright citizenship affect immigrant children's participation in non-compulsory preschool education and their developmental outcomes at this

[10] For an overview of ethnic education inequalities across European countries, see Dustmann *et al.* (2012).

educational stage? Did it accelerate their progress through primary school? And was there an effect on immigrant children's selection into different secondary school tracks? Taken as a whole, the answers to these questions allow us to gauge the extent to which granting citizenship to immigrant children affects their educational outcomes both in the short and the longer term.

The citizenship reform we examine was introduced in Germany at the turn of the millennium; it essentially constituted a change from *jus sanguinis* (right of blood) to *jus soli* (right of soil). Under *jus sanguinis*, only descendants of home-country nationals receive citizenship. By contrast, under *jus soli*, every individual born on the national territory is eligible for citizenship.[11] The German reform allows us to exploit one particular feature:[12] all children born to foreign parents from 1^{st} January 2000 onward were automatically granted citizenship if at least one parent had been a legal resident in Germany for eight or more years at the time of birth. This setting provides us with a birth date cut-off regarding the entitlement to automatic birthright citizenship. We rely on this exogenous source of identification and employ a difference-in-differences design (DiD) which not only compares immigrant children born shortly before and shortly after the cut-off date, but also draws upon immigrant children from the preceding birth cohort as a control group – an approach that allows netting out possible age and seasonal differences between children born in different semesters.

Two unique administrative data sets covering the student population of one federal state of Germany, Schleswig-Holstein (a state with 2.9 mio. inhabitants), allow us to implement our empirical approach and to provide answers to the questions raised at the outset. First, we use administrative records from school entry examinations. These records contain physicians' assessments of children's language proficiency, socio-emotional maturity and overall school readiness at age six. An accompanying questionnaire, filled out by parents, provides (retrospective) information on children's preschool enrollment and family background. Our baseline analysis relies on the records of immigrant children born between July 1998 and June 2000 and examined for school entry in 2005 and 2006.

[11] Birthright citizenship has historically been in place in the United States, Canada and the United Kingdom, but has only recently been introduced in some European countries (e.g., Belgium, Greece, Germany) while in others its introduction is currently being discussed (e.g., Italy).

[12] The reform also changed other features of Germany's nationality law, which we discuss in Section 3.3.1.

Second, we rely on administrative school registers. These registers contain information on children's progress through primary school, primary teachers' track recommendations and children's selection into secondary school track. We use school registers from 2009 and 2010, the time when the cohorts under study were scheduled to start secondary school.

Besides being one of the main immigrant destination countries, studying the case of Germany has at least one further advantage. In the German education system, there are essentially three parental decisions to be made: (i) whether to enroll a child in *preschool*; (ii) when to enroll a child for compulsory *primary school*; and (iii) which educational track a child shall follow in *secondary school*. In making the latter two decisions, parents can draw upon official recommendations. In particular, prior to primary school enrollment, pediatricians assess a child's "school readiness" and may recommend deferred school entry. In a similar vein, teachers make a recommendation for a child's secondary school track in the final year of primary school. In several federal states, among them Schleswig-Holstein, official recommendations are meant to guide parental decision-making, but are non-binding, i.e., the educational decisions are at parents' discretion. As such, we are able to ask whether (possible) effects of the reform are driven by changes in official recommendations, parental educational choices, or a combination thereof.

We obtain three sets of results related to the different educational phases. The key result for the *preschool* period is that the introduction of birthright citizenship led to an increase in immigrant children's enrollment in non-compulsory preschool by 3.4% – an effect which appears to close the enrollment gap between native and immigrant children completely. Turning to developmental outcomes measured at the end of the preschool period, we find a significant increase in immigrant children's German language proficiency as well as their socio-emotional maturity (by 6.0% and 2.1%, respectively). During the *primary school* phase, our results suggest the following impacts of birthright citizenship: the average school starting age of immigrant children decreases by 1.6 months, which translates in more than a doubling of the incidence of early school entry. As regards educational progress, we find a reduction in the probability of grade repetition among immigrant children by 24.6% – an effect which corresponds to a closure of the immigrant-native gap by one half. Finally, and most importantly for immigrants' longer-term out-

comes, we detect a significant increase in immigrant children's probability of attending the academic track of *secondary school* by 49.0%. This effect reduces the immigrant-native academic-track enrollment gap by almost one-half. Moreover, it is not only driven by improved teachers' track recommendations for immigrant children, but also by an adjustment of immigrant parents' secondary school track choices. In particular, we observe an increase in the proportion of immigrant children attending the academic track of secondary school *without* an official recommendation by 52.3%.

We view our study as making three contributions to the literature. First, we contribute to a growing literature identifying the causal determinants of immigrant children's educational integration. In contrast to most existing studies, which employ decomposition methods to analyze factors responsible for the educational gap between immigrant children and their native peers (see Section 3.2 for an overview), we make use of an exogenous policy variation. Second, we recognize that education is a dynamic process that proceeds in stages and provide a nuanced picture of the effects of birthright citizenship as immigrant children go through the first three critical stages of this process (i.e., preschool, primary school and secondary school). In contrast, previous work has typically focused on one stage of this process in isolation. In fact, much of the literature examining ethnic educational gaps relies on data from the Program of International Student Assessment (PISA) and thus focuses on the secondary education stage only (e.g., Dustmann *et al.*, 2012).[13] Finally, our setting allows us to study immigrant parents' choices regarding their children's education. As such, we contribute to the current political and scientific discussion on how to overcome one of the main obstacles to immigrant children's educational integration: their parents' behavior which is frequently characterized by a lack of

[13]The same is true for a contemporaneously written technical report by Clots and Sajons (2014). Although the issues we are interested in are similar to those explored in Clots and Sajons (2014), our approach differs markedly from theirs. Most importantly, the administrative data we have at hand allow us to study the reform's effect from an early life cycle perspective, i.e., across the first three critical stages of the German education system. Clots and Sajons (2014) draw upon survey data from the National Education Panel Study (NEPS) and focus on the secondary school stage in isolation. Second, we analyze not only children's educational outcomes, but examine simultaneously the role of official recommendations as well as parents' decisions for immigrant children's educational integration. Last, but not least, our identification strategy compares immigrant children born shortly before and after the reform's cut-off date, and draws upon immigrant children from adjacent birth cohorts as a control group. In contrast, the identification strategy chosen by Clots and Sajons (2014) relies on a comparison between immigrant and native children, a group for which it is unclear whether the underlying identifying assumption – a stable difference in educational outcomes between children born in different semesters – holds.

effective interactions with schools and other institutions that promote the development of their children (Diehl *et al.*, 2016).

The remainder of the chapter is organized as follows. In the next section, we review the related literature and discuss potential mechanisms underlying the effect of birthright citizenship on immigrant children's educational integration. Section 3.3 describes the institutional background. Section 3.4 introduces the empirical strategy. Section 3.5 describes the data used for the analysis. Section 3.6 presents the main results and provides a series of sensitivity checks. Section 3.7 concludes.

3.2 Related Literature and Potential Mechanisms

There is a large literature studying the educational inequalities between immigrant and native children, both from a cross-national and a within-country perspective (see Diehl *et al.* (2016) and Suárez-Orozco *et al.* (2015) for recent overviews). Across OECD countries, there is considerable heterogeneity in immigrant children's educational achievements (see, e.g., Algan *et al.*, 2010; Entorf and Minoiu, 2005). In countries where immigrant children are, on average, outperformed by their peers (e.g., in Continental Europe), the achievement gap is associated with factors such as family background, language spoken at home and specific features of the educational system, such as tracking (e.g., Akresh and Akresh, 2011; Dustmann *et al.*, 2012; Lüdemann and Schwerdt, 2013; Schnepf, 2007).[14]

An emerging literature deals with the integrative impact of educational policies. A few studies provide a picture of the beneficial impacts of preschool on the educational achievement of immigrant children (Drange and Telle, 2017; Cornelissen *et al.*, 2016; Felfe and Huber, 2015). (Currie and Thomas, 1999) refer specifically to the quality of preschools and show that participation in *Head Start* – a high quality preschool program in the United States – closes one fourth of the gap in test scores between hispanic children and non-hispanic white children, and two-thirds of the gap in the probability of grade retention. Some other related studies conduct cross-country comparisons of test scores

[14] An additional factor for the low educational attainment of certain immigrant groups (e.g., hispanic children in the US) is racial segregation and the associated low quality of the schools disproportionably frequented by immigrants (Wells, 2009). This factor is less relevant for Europe (Dronkers and Levels, 2007), as the quality variation between European schools is smaller than that of schools in the United states (Scheerens and Bosker, 1997).

resulting from PISA and relate better test performance to school systems with reduced ability tracking and a low share of private schools (Cobb-Clark *et al.*, 2012; Ruhose and Schwerdt, 2016; Schneeweis, 2011).

We instead focus on the impact of one increasingly common, yet still contentious integration policy: *birthright citizenship*. In what follows, we highlight some potential mechanisms explaining why and how birthright citizenship may influence immigrant children's educational integration.

Citizenship improves a person's legal position by allowing for full political participation, thus improving a person's standing in society. Citizenship also improves professional opportunities. First, in Germany, it opens the door to any job requiring civil servant status.[15] Second, there is evidence that naturalized immigrants earn more than non-naturalized ones (Chiswick, 1978; Steinhardt, 2012), have higher job-finding rates (Fougère and Safi, 2009; Gathmann and Keller, 2017) and experience steeper wage-tenure profiles (Bratsberg *et al.*, 2002).[16] Thus, it can be argued that the introduction of birthright citizenship in Germany represents a positive shock to the long-run rate of return to education for immigrant children.

This, in turn, may trigger human capital investments by parents that foster immigrant children's educational integration in the short- as well as in the long-run. First, birthright citizenship may stimulate parents' cultural and social assimilation efforts. Indeed, exploiting the same reform as we do, Avitabile *et al.* (2013) and Sajons (2012) provide evidence that foreign-born parents are more likely to interact with the local community and use the language of the host country if their children are entitled to citizenship at birth.[17] Second, Avitabile *et al.* (2014) find that birthright citizenship leads to a reduction in immigrant fertility and thus potentially frees up parental time available for each child. In line with the theoretical predictions of the quantity-quality model put forward by Becker and Tomes (1976), they furthermore provide evidence on positive short-run

[15] Civil servant posts in Germany are either prestigious (e.g., teachers or judges), or include jobs that might appeal to children, such as firefighters or police officers.

[16] That said, with the exception of Gathmann and Keller (2017), these studies cover periods with no exogenous policy variation.

[17] Yet, they do not find any effect on parental naturalization rates nor on parents' socioeconomic status (e.g., by stimulating their employment or earnings) which may act as further stimuli to children's development.

effects on behavioral and health outcomes among immigrant children affected by the reform of the German naturalization law.

Whether birthright citizenship leads to human capital investments that improve immigrant children's educational outcomes in the longer-term is, however, still an open question. In fact, it is far from clear whether the observed initial parental assimilation efforts and investments translate into visible improvements in children's educational achievements. In light of a human capital development framework, two scenarios are conceivable:[18] on the one hand, parental assimilation may result in educational decisions that are similar to the ones made by natives – e.g., increased preschool enrollment or academic track enrollment in secondary school – and result in improved child outcomes in the short-run and in the long-run. On the other hand, if initial assimilation and investment do not yield the expected returns and/or are not reflected in improved official recommendations, it may well be the case that parental efforts fade out over time and no lasting effect on immigrant children's education is observed. In summary, whether birthright citizenship has a lasting impact on immigrant parents' educational decision-making and their children's subsequent educational progress is an empirical question for which we provide some answers.

3.3 Institutional Background

3.3.1 Reform of the German Nationality Law in 1999

In May 1999, the German parliament undertook a major revision of the "German Citizenship and Nationality Law" dating back to 1913. On 15^{th} July 1999, the German parliament ratified the new version of the law, which included two major changes: (1) it changed the eligibility criteria for naturalization; and (2) it introduced birthright citizenship.

This project focuses on the second change. Until 1999, citizenship was granted according to *jus sanguinis*, i.e., children became German citizens only in cases where at least one parent held the German citizenship. As of 1^{st} January 2000, the prevailing regime changed to *jus soli*, granting each child born on German territory a *conditional* right to

[18] For an overview of the human capital production theory, see Cunha *et al.* (2006)

German citizenship at birth. The conditionality attached to birthright citizenship was that at least one parent had been a legal resident in Germany for eight years or more at the time of birth. Upon satisfying this condition, German citizenship was *automatically* recorded in the register of birth with no need for the parents to apply for it. There were no known cases of refusal of German citizenship at birth, not at last due to the allowance of dual citizenship until age 23.

Parents of children born between 1991 and 1999 could take advantage of a transition rule: conditional on having legally resided in Germany for at least eight years, they could retrospectively apply for their children's citizenship within a transition period of one year (1^{st} January to 31^{st} December 2000). However, only a small fraction of eligible families made use of this transition rule. In what follows we present evidence showing that the number of eligible children born in 1999 and whose parents made use of the transition rule is only about one-seventh of the number of eligible children born in 2000 who were automatically granted citizenship. The main reason why only a small fraction of families made use of the transition has to do with a lack of diffusion of information. On the one hand, families were not directly informed about the transition rule. On the other hand, the public discussion mainly revolved around the two main aspects of the reform.

As mentioned above, the reform also changed the path to citizenship for adults. Importantly, this aspect of the reform applied to *all* immigrant adults and thus to *all* immigrant parents, i.e., independently of whether their children were endowed with birthright citizenship or not. While the reform relaxed the length-of-residence requirement from 15 years down to eight, it tightened the requirements regarding German-language proficiency and explicitly forbade dual citizenship (after age 23). Thus, despite the relaxed residence requirement, the costs and effort associated with a citizenship application increased. Hence, while in principle granting children citizenship could have induced parental naturalization, in practice it did not (Avitabile *et al.*, 2013; Sajons, 2012).

3.3.2 The German Education System

The German education system is comprised of three parts: (1) the phase between birth and primary school, which is divided into early care available for children up to age 2 and preschool available for children aged 3 to 5 years; (2) primary school, which usually

starts at age 6 and continues for four years; and (3) secondary school, which typically starts at age 10 and continues for between five (compulsory) and nine years.

Preschool attendance is non-compulsory in Germany. Notwithstanding this, since 1996, every child turning 3 years old has been legally entitled to a slot in preschool. As a result, the supply of preschools rose dramatically in late 1990s and ultimately met demand the early 2000s. Preschool is heavily subsidized on an income sliding scale. In case of severe financial constraints, fees can be reimbursed by the local youth welfare service (Bundesministerium für Familie, Senioren, Frauen und Jugend, 2013). In 2012, 96 % of all 3-5 year-old native children attended preschool, while the corresponding share of immigrant children was 87 % (Bildungsberichterstattung, 2012).

Between birth and primary school, children undergo several compulsory medical screenings. These medical screenings are intended to document children's health, diagnose medical anomalies, and provide necessary treatment as early as possible. An important medical screening is the school entry examination, which is offered by the local health service and takes place in the year prior to entering primary school. In particular, all children turning six years old between July of the year prior to enrollment and June of the same year as enrollment are subject to the school entry examination. In addition to documenting a child's health, pediatricians determine whether a child is "ready" to follow the school curriculum. The school readiness diagnosis is an important factor in school enrollment: a negative assessment can lead to school entry being deferred by one year. Yet, the ultimate decision when to enroll a child for primary school lies in the hands of parents.

After passing all four grades of primary school, students are referred to secondary school (around age 10). Secondary school is divided into the following three tracks:[19] the lowest track (the so-called *Hauptschule*) which continues up to Grade 9 or 10 and gives student a general education in preparation for an apprenticeship; the intermediate track (*Realschule*) which goes up to Grade 10 and can either lead to an apprenticeship or to a higher-level vocational school; and the academic track (*Gymnasium*) which goes up to Grade 12 or 13 and prepares students for university.

[19]There are also a number of alternative school types, such as the Waldorf schools and the comprehensive school (*Gesamtschule*). Overall, around 10 % of all children of secondary school age attend alternative types of schools.

The transition from primary school to the different secondary school tracks is a critical stage in the German education system. In the final year of primary school, teachers make a recommendation which secondary school track their students should enter. These recommendations are supposed to reflect students' abilities and not their socioeconomic background. During our study period, the ultimate track choice was at the discretion of the parents in 10 out of 16 German federal states, among them Schleswig-Holstein.[20] In 2009, 24.1 % of all 15-year-old immigrant children attended *Hauptschule*, in comparison to 13.3 % of their native peers. In contrast, 25.9 % of all 15-year-old immigrant students attended *Gymnasium*, whereas 37.1 % of their native peers did so (Bildungsberichterstattung, 2012). In other words, the unconditional immigrant-native enrollment gap in the academic track amounted to 11.2 percentage points (ppt). Lüdemann and Schwerdt (2013) show that half of this gap remains after accounting for children's performance during primary school, which might be due to discrimination by teachers or lower educational aspirations among immigrant families (or a combination thereof).

3.4 Empirical Approach

3.4.1 Identification Strategy

We estimate the causal effect of automatic birthright citizenship on a range of educational indicators. Our identification strategy rests on a difference-in-differences (DiD) design which exploits the cut-off date of the German citizenship reform (i.e., 1^{st} January 2000). In particular, we first compare the educational outcomes of immigrant children who were born shortly before and shortly after the cut-off date. To avoid differences across school cohorts, we restrict our attention to one school cohort, i.e., we compare children born six months before and after the cut-off date. A key concern with this simple difference is that the characteristics of parents and children might systematically vary on the two sides of the cut-off date. First of all, children belonging to one school cohort differ by up to 12 months in age, with children born in the earlier months likely to be more mature at any given point in time. In addition, there is evidence that children born in spring

[20] In the remaining states, a child either has to pass a special exam or undergo a probationary period in case it wants to attend *Gymnasium* without a recommendation.

come from more advantaged socioeconomic backgrounds than children born in winter (Buckles and Hungerman, 2013). In order to avoid biased estimates due to such age and seasonal effects, we use immigrant children from another school cohort as control group. In particular, our control is comprised of immigrant children born in the 12-months window centered around 1^{st} January 1999.[21] Notice that these children were all born under the old *ius sanguinis* regime and no policy change occurred at this control cut-off date.

Our regression model can be written in the following way:

$$\begin{aligned} Y_{i,m} =& \alpha + \beta \text{Treated Cohort}_{i,m} + \gamma \text{After}_{i,m} + \delta \left(\text{Treated Cohort}_{i,m} \times \text{After}_{i,m}\right) \\ &+ \sum_{m} \theta_m D_{i,m} + \epsilon_{i,m} \end{aligned} \tag{3.1}$$

where $Treated\ Cohort_{i,m}$ is a binary variable indicating whether child i born in month m belongs to the school cohort that was subject to the policy change (i.e., it switches on for children born between July 1999 and June 2000). The coefficient β captures any differences between immigrant children born in the treated and the control cohort. $After_{i,m}$ is a binary assignment variable indicating whether child i was born in the months just after 1^{st} January of a given year (i.e., it switches on for children born between January and June); it captures differences (e.g., due to age or seasonal effects) between children born in the different semesters of a school cohort. To allow for any heterogeneity of the age and seasonal effects that may arise because children are born in different months, we furthermore control for a full set of month-of-birth dummies $D_{i,m}$.[22] Finally, we include the interaction between $Treated\ Cohort_{i,m}$ and $After_{i,m}$ which switches on for children born between January and June 2000 and thus for all children born after the policy change and thus under the newly-introduced *ius soli* regime. The corresponding coefficient δ is

[21] Similar identification strategies have been used by Lalive and Zweimüller (2009), Dustmann and Schönberg (2012), Danzer and Lavy (2016), and Schönberg and Ludsteck (2014) in the context of parental leave reforms.

[22] The assignment variable $After_{i,m}$ correlates perfectly with the birth months January to June. We therefore omit not just one, but two birth month dummies. Specifically, we omit January and December as they are immediately around the cutoff date. Note that we can control for the set of birth months dummies because we rely on a comparison between children born in the year of policy change and children born in the preceding year in which there was no policy change. A simple regression discontinuity design would not allow us to do so, as the assignment variable would be a perfect linear combination of the included set of birth month dummies.

the parameter of interest, whose interpretation we discuss below. We estimate equation (1) separately for various educational indicators measured at the first three stages of the German education system. Standard errors $\epsilon_{i,m}$ are clustered at the birth month/year level.

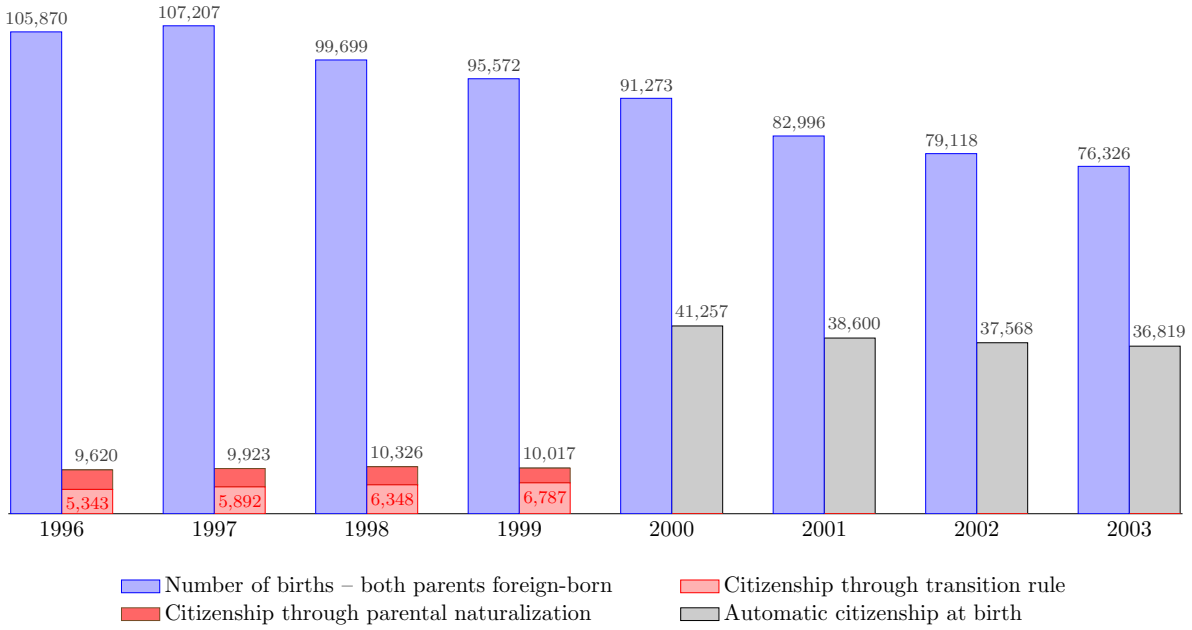
3.4.2 Interpretation of Estimates

The Effects of Automatic Birthright Citizenship vs. Optional Citizenship.— It is important to be clear upfront that δ does not identify the effects of citizenship on immigrant children's educational integration. Instead, it captures the impact of a policy switch from *optional* citizenship for immigrant children (under the "old" *jus sanguinis*) to *automatic* birthright citizenship (under the "new" *jus soli*). Children fulfilling the eligibility criteria and born after the policy change on 1^{st} January 2000 were automatically granted citizenship at birth. In contrast, eligible children already born at the time of the policy change could become German citizens retrospectively in cases where parents made use of the one-year transition rule or where at least one of the parents chose to naturalize him or herself. As already mentioned above, the former option was hampered by informational constraints, while the latter was associated with substantial costs for the parents, most importantly the costs of giving up the citizenship of their country of origin. Thus, our empirical approach centers around the comparison of a regime in which citizenship was conferred automatically and costlessly to children born to foreign parents versus a regime in which immigrant parents had the costly option of obtaining citizenship for their children. For brevity, we will refer to δ as the *causal effect of automatic birthright citizenship*.

In order to estimate δ, one would ideally restrict attention to eligible children whose parents had the necessary years of residence (≥ 8 years) when the reform came into effect. Since our main data sources do not contain information on length of parental residence, we are not able to restrict our estimation samples to eligible children. As a result, the regressions we run also cover children who were unaffected by the reform. Thus, our estimates of δ capture the reform's intention-to-treat (ITT) effect. This ITT effect will be a conservative estimate of the reform's average treatment effect on the treated (ATT), since it is estimated on a sample which includes children who are all born to foreign

parents who may or may not not fulfill the residency criterion and hence may or may not qualify for birthright citizenship. To provide some numbers regarding the share of eligible children and the share of ineligible children, we combine official citizenship data of the Federal Office for Migration and Refugees with official birth statistics of the Federal Statistical Office (see Figure 3.1): among the 91,273 children born to foreign nationals in Germany in the year 2000, 41,257 were automatically granted citizenship at birth. Thus, the fraction of eligible children in the cohort subject to the policy change was 45.2 %. This suggests that the reform's ATT is in the order of roughly 2.2δ.

Figure 3.1: Children born to foreign-born parents, eligible children and children with citizenship



The Effects of Citizenship.— As an alternative to identifying the reform's impact in an ITT setting, one could ask: What is the causal effect of citizenship on immigrant children's educational integration? Our estimates of δ would identify this effect if the birth-date cut-off perfectly determines whether a child obtains the German citizenship or not. Since this is not the case, one is left with the possibility to exploit the birth-date cut-off as a source of exogenous variation for a child's citizenship and to employ an instrumental variable approach. Yet, limitations of the data under study – the lack of information on citizenship – prevent us from taking such an approach. Data limitations are also the reason why we can not pursue a two-sample instrumental variables approach:

available survey data suffer from misreporting of children's citizenship and thus prevent us from obtaining credible first-stage results. For example, in the 2003 and 2004 waves of the German Microcensus – the earliest waves which contain detailed information on individual month-of-birth –, only 67 % of all eligible immigrant children born in the year 2000 are reported to possess the German citizenship. One reason for why this share is not equal to 100 % is that parents are likely to report their own nationality when asked about their child's citizenship despite the fact that their child possesses dual citizenship. That said, the evidence presented in Figure 3.1 gives some idea about the first-stage effect and how to calculate a back-of-the-envelope estimate. In particular, while 45.2 % of all children of foreign citizens born in 2000 where automatically granted citizenship at birth, only 10.5 % of those born in 1999 received German citizenship in the wake of the reform (7.1 % via the transition rule and 3.4 % through parental naturalization[23]). Thus, children born in the year after the reform were roughly 35 ppt more likely to hold German citizenship as infants than children born in the year prior to the reform. To put it differently, the first stage estimate likely amounts to a value of 0.35. We consider this as suggestive evidence that the local average treatment effect of citizenship on immigrant children's educational integration is in the order of roughly 2.8δ.

3.4.3 Threats to Identification

There are several potential threats to identification. The major concern is that reform had a direct effect on the composition of children born shortly before and shortly after the reform. While descriptive evidence speaks against this concern (see Table 3.1 for details), in what follows we discuss the reasons for potential compositional changes and how we deal with the arising threats.

Fertility Behavior.— The German citizenship reform may have induced immigrant parents to adjust their fertility behavior in three dimensions. First, parents may have delayed conception to ensure that their child is born under the new *ius soli* regime.

[23]We have calculated the number of children obtaining the German citizenship through parental naturalization by proxying the naturalization rate of immigrant parents giving birth in year t with naturalization rate of all immigrants in year t. The annual naturalization rates of immigrants are calculated by the Federal Statistical Office and are given by: 4.04 % (1996), 3.76 % (1997), 3.99 % (1998), and 3.28% (1999).

Second, and as mentioned above, the introduction of birthright citizenship is likely to have enhanced children's future labor market opportunities and thus have increased the perceived "quality" of children. According to the quantity-quality tradeoff emphasized by Becker and Tomes (1976), this may have caused immigrant parents to adjust their desired number of children downwards. Avitabile *et al.* (2014) provide evidence that the German citizenship reform indeed led to a reduction in immigrant fertility, but only from 2001 onward. Hence, the children included in our sample, who were conceived before September 2000, are unlikely to be affected by this concern. In fact, a histogram of the number of immigrant children born around the cut-off date does not reveal any discontinuity (see Appendix Figure B.1). Notwithstanding this, we test the robustness of our results to restricting our sample to children who were conceived before the new German naturalization law was ratified, i.e., to children conceived before July 1999 and thus born by April 2000. This leaves us with a 8-months window centered around the cut-off date. Finally, mothers scheduled to give birth shortly before the cut-off date may have tried to postpone the birth date in order to benefit from *ius soli*. Although this type of behavior is difficult to implement, we perform a robustness check in which we exclude children born in the months right before and after the cut-off date (i.e., December 1999 and January 2000) – the so-called donut-strategy.

Return Migration.— A second concern is that the introduction of birthright citizenship made return migration less attractive. As a result, the pool of families remaining in the country may change after the introduction of birthright citizenship. As mentioned above, we do not observe any significant differences in the available parental background characteristics between immigrant children born shortly before and after the cut-off date (see Table 3.1). Differences in background characteristics not captured in our data may, however, still be a threat to identification. In fact, Sajons (2016) argues that the reform likely induced more families, in particular families from the lower end of the socio-economic distribution, to stay in Germany. Yet, in this case any differences would lead to an underestimation of the effect. Nevertheless, to test the robustness of our results to potential sample selection bias, we extend our baseline specification and include a series of family background characteristics, such as single parenthood, parental education and

parents' country of origin. Notice that this is only possible in two out of three data sets used in this study (see Section 3.5 for more details).

Miscellaneous.— Finally, we address potential concerns about remaining differences between children born in the different semesters of a school cohort that may not be captured by the preceding cohort by adding yet another school cohort to the control. Specifically, we draw upon the adjacent post-reform cohort, children belonging to the cohort born in the 12-months window around 1^{st} January 2001, and control flexibly for cohort dummies as well as their interactions with birth semester.

3.5 Data

The questions we ask – and how we answer them empirically – require very comprehensive data. First, we need information on immigrant children throughout the first three stages of the German education system. Second, our identification strategy relies on a very small subgroup of the German population: a few cohorts of immigrant children. We therefore draw upon two unique administrative data sources available in a suitable way in the German federal state of Schleswig-Holstein: school entry examinations and school registers. Both data sources allow us to proxy immigrant status. However, both data sources lack information necessary to infer whether or not immigrant children are eligible for automatic birthright citizenship: parents' length of residence in Germany and in the case of the school entry examinations also children's country of birth. To gain insights into the potentially arising selection due to the lack of this information, we use the German Microcensus as an additional data source. We now describe these three data sets in detail.

3.5.1 School Entry Examination

Data on educational outcomes during the preschool phase are contained in the administrative records from the so-called school entry examination (SEE). This examination is compulsory for all children who turn six in the 12-months period before a new school year begins. Children born between July 1998 and June 2000 are thus included in the

SEEs 2005 and 2006. The SEE records contain several measures of child development (e.g., language proficiency, behavior problems, emotional stability, social skills) as well as physicians' assessments of children's school readiness. Parents fill out an accompanying questionnaire about their children's previous preschool enrollment and their own background. As for the latter, the questionnaire contains information about parents' country of birth, but not about their nationality and length of residence in Germany. As a result, we are unable to restrict our sample to eligible children. In fact, all we know is whether a child's family was living in Germany at the time of the survey, but not whether the child was born in Germany. Data from the German Microcensus, however, suggest that 85 % of all children with foreign-born parents who reside in Germany at the age of 6 were born in Germany. In other words, the scaling factor for our ITT estimates based on the SEE records needs to be adjusted upwards from 2.2 to 2.6.

In summary, we restrict our attention to children whose parents were both not born in Germany. This sample restriction leaves us with 4,450 observations including both first-generation immigrant children as well as second-generation immigrant children who may or may not be eligible for birthright citizenship. The SEE records allow us to construct the following binary dependent variables (some of them may not be available for all counties of Schleswig-Holstein – either because they have not been surveyed or not been transferred to the central authority in charge of data collection):

- *Preschool Enrollment:* equal to one for children that have attended preschool and zero otherwise (available in all counties/ no missings).

- *Language proficiency:* equal to one for children who are fluent in German or make at most small mistakes and zero otherwise (available in 2005 for only 1 out of 15 counties and in 2006 for 11 out of 15 counties/ missing for 72% of the sample).

- *Socio-Emotional Maturity:* a combined measure of several developmental deficits – attention deficits, emotional instability (e.g., anxiety, nervousness, lack of self-confidence), antisocial and deviant behavior (e.g., lying, fighting, stealing) – which is equal to one if none of the mentioned deficits is diagnosed and zero otherwise (available in 11 out of 15 counties/ missing for 22%).

- *School Readiness:* equal to one for children assessed to be ready to follow the primary school curriculum and zero otherwise (available for all counties/ no missings).

Table 3.1 displays descriptive statistics from the SEE records for both native and immigrant children born prior to the reform as well as immigrant children born around the introduction of the reform. In terms of the dependent variables (panel A), a raw comparison between children born shortly before (II/99) and shortly after (I/00) the cut-off date suggests several differences. The preschool enrollment rate of immigrant children born after the cut-off date comes close to that of their native peers (94.2% vs. 95.4%), but it is roughly 3 ppt lower for immigrant children born shortly before the cut-off (91.7%).

Table 3.1: Summary statistics: school entry examination (SEE)

	Natives Pre-reform	Migrants Pre-reform	Migrants II/99	Migrants I/00	Diff I/00 - II/99
		A. Dependent Variables			
Preschool Enrollment	0.954	0.925	0.917	0.942	0.025**
Language Proficiency	-	-	0.580	0.504	-0.077***
Socio-Emotional Maturity	0.904	0.918	0.902	0.903	0.001
School Readiness	0.909	0.849	0.863	0.772	-0.091***
		B. Background Variables			
Age in Months	75.176	73.805	76.396	70.724	-5.672***
Female	0.473	0.468	0.478	0.498	0.020
Nr. of Siblings	0.869	1.207	1.287	1.257	-0.030
Single Parent	0.131	0.085	0.088	0.087	-0.001
Mom's Education: Low	0.200	0.217	0.220	0.235	0.014
Mom's Educ.: Intermediate	0.332	0.245	0.240	0.240	0.000
Mom's Educ.: High	0.234	0.164	0.154	0.169	0.015
Mom's Educ.: Missing	0.234	0.374	0.386	0.356	-0.030
Dad's Education: Low	0.236	0.235	0.236	0.222	-0.014
Dad's Educ.: Intermediate	0.229	0.222	0.229	0.240	0.011
Dad's Edu.: High	0.266	0.168	0.154	0.181	0.027
Dad's Educ.: Missing	0.269	0.375	0.381	0.357	-0.024
Mom's Origin: Turkey	-	0.315	0.306	0.315	0.009
Mom's Origin: East Europe	-	0.389	0.405	0.395	-0.011
Mom's Origin: Balkan	-	0.085	0.082	0.074	-0.008
Mom's Origin: EU 12	-	0.048	0.045	0.050	0.005
Mom's Origin: missing	-	0.166	0.162	0.166	0.004
Dad's Origin: Turkey	-	0.319	0.311	0.316	0.005
Dad's Origin: East Europe	-	0.377	0.393	0.376	-0.016
Dad's Origin: Balkan	-	0.087	0.081	0.077	-0.004
Dad's Origin: EU 12	-	0.047	0.042	0.051	0.009
Dad's Origin: Missing	-	0.170	0.173	0.180	0.007

Notes: *Migrants Pre-reform* refers to children born between June 1998 and December 1999 and whose parents were both not born in Germany. *Migrants II/99* refers to immigrant children born between July and December 1999. *Migrants I/00* refers to immigrant children born between January and June 2000. *Natives Pre-reform* refers to all non-immigrant children born between June 1998 and December 1999.

In terms of children's socio-emotional maturity no significant differences between children born shortly before and after the cut-off date are observed, but controlling for age effects might well change this picture. The significantly higher probability of being assessed as "fluent in German" and "ready for school" among children born before the

cut-off is in fact likely to reflect such age effects. And indeed, the descriptive statistics on background variables (panel B) show that the average age difference between the two groups of immigrant children amounts to 5.7 months. Aside from this age gap, there are no significant demographic and socioeconomic differences between immigrant children born shortly before and after the cut-off date – a fact which supports the assumption of comparability between the children born before and after the cut-off date.

3.5.2 School Registers

Educational outcomes during the primary and secondary school phase are taken from school register (SR) data. In Schleswig-Holstein, all primary and secondary schools are legally obliged to provide individual student records to the federal ministry of education at the beginning of each school year (i.e. September). The two birth cohorts we study were scheduled to be admitted to secondary school in 2009 and 2010, respectively. We therefore draw upon the records from these two years.

The SR data provide basic information about children's gender and birthdate. In addition, they include two variables that allow us to proxy children's migrant status: country of birth and main language spoken at home. We restrict our sample to children who are born in Germany, but use a language different from German as their first language at home.[24] Particularly the latter restriction is likely to exclude a substantial share of immigrant children – in fact, despite the fact that they are based on the same population, the sample based on the SR records from 2009 and 2010 contains substantially fewer observations than the sample based on the SEE records from 2005 and 2006 (1,611 versus 4,450 observations). Calculations based on the German National Educational Panel Study (NEPS) suggest that slightly less than half of all children with foreign-born parents do not speak German at home[25]. Further inspections of the NEPS indicate that this subsample of immigrant children consists of disproportionately more children from disadvantaged backgrounds and from Turkish families. To address the concern that our baseline estimates may be sample-specific, we conduct additional analyses using the

[24]We additionally exclude children who are ethnic Germans ("Aussiedler") and thus possess of German citizenship by definition

[25]In contrast, in the NEPS data only a negligible fraction of children whose parents were both born in Germany speak a language other than German at home.

German Microcensus. As already discussed, this allows us furthermore to address issue of children's eligibility for birthright citizenship, an issue also present in the SR data.

The SR data allow us to construct the following dependent variables for the *primary school* phase:

- *Age at School Entry:* continuous variable measuring the age of children in months at the time of primary school entry.
- *Early School Entry:* equal to one for children who have entered primary school prior to the scheduled year of admission and zero otherwise.
- *Grade Retention:* equal to one for children who have repeated one or more grades during primary school and zero otherwise.

As for *secondary school* education, we are able to draw upon the following dependent variables:

- *Recommendation:* equal to one for children who have received the official recommendation to attend the academic track of secondary school and zero otherwise.
- *Gymnasium:* equal to one for children who are enrolled in the academic track of secondary school.
- *Gymnasium with Recommendation:* equal to one for children who are enrolled in the academic track of secondary school based on the recommendation from their primary school teacher.
- *Gymnasium w/o Recommendation:* equal to one for children who are enrolled in the academic track of secondary school despite no recommendation from their primary school teacher.

Table 3.2 presents descriptive statistics from the SR data for both " native" – comprising all children speaking German at home and thus also a substantial share of immigrant children – and immigrant children – consisting, as discussed above, of children not speaking German at home and thus likely being a selected subsample of all immigrant children. Compared to their native peers, immigrant children are less likely to enter primary school

ahead of the scheduled year of admission (5.2% vs. 8.2%), are more likely to be retained (27.1% vs. 15.6%) and less likely to receive a recommendation for the academic track of secondary school (13.1% vs. 39.1%). Based on the SR data, the actual immigrant-native enrollment gap in the academic track of secondary school amounts to 20.0 ppt (16.3% vs. 36.3%) – a gap which is substantially higher than the officially reported gap which amounts to approximately 11.2 ppt (25.9% versus. 37.1%). This difference is in line with the issue discussed above that our SR sample is disproportionately made up of children from disadvantaged backgrounds.

Table 3.2: Summary statistics: school registers (SR)

	Natives Pre-reform	Migrants Pre-reform	Migrants II/99	Migrants I/00	Diff I/00 - II/99
	A. Dependent Variables: Primary School				
Age at School Entry	79.843	80.491	76.906	83.974	7.068***
Early School Entry	0.082	0.052	0.086	0.019	-0.067
Grade Retention	0.156	0.271	0.236	0.305	0.069**
	B. Dependent Variables: Secondary School				
Recommendation for Gymnasium	0.391	0.131	0.154	0.105	-0.048**
Gymnasium	0.363	0.163	0.194	0.132	-0.062**
– with Recommendation	0.293	0.087	0.108	0.067	-0.041**
– w/o Recommendation	0.070	0.075	0.086	0.065	-0.021
	C. Background Variables				
Age in Months	126.400	126.506	123.410	129.573	6.163***
Female	0.490	0.494	0.506	0.482	-0.024

Notes: *Migrants Pre-reform* refers to children born in Germany between June 1998 and December 1999 and use a language different from German as their main language at home. *Migrants II/99* refers to immigrant children born between July and December 1999. *Migrants I/00* refers to immigrant children born between January and June 2000. *Natives Pre-reform* refers to all non-immigrant children who were born in Germany between June 1998 and December 1999. Notice, however, that given our definition of *natives* – everyone who speaks German at home – this group likely comprises a non-negligible number of immigrants. As such, the reported immigrant-native differences are likely to differ from gaps based on alternative statistics.

A comparison between immigrant children born shortly before and shortly after the cut-off date suggests several differences: children born before the cut-off start school at an earlier age (77 vs. 84 months), repeat more often a grade during primary school (23.6% vs. 30.5%), receive more frequently a positive recommendation for the academic track of secondary school (15.4% vs. 10.5%), which finally results in a higher enrollment rate (19.4% vs. 13.2%). As previously discussed these differences are likely driven by the imminent age difference and underlines the importance of using a difference-in-differences approach netting out the effects of age. Unfortunately, the SR does not provide detailed background information and thus prevents us from conducting useful balancing tests between immigrant children born shortly before and after the cut-off date. Yet, analysis based on the German Microcensus, introduced in the next section, does not reveal any significant differences between children born before and after the cut-off date, except the previously discussed differences in children's age and number of siblings (see Table B.1 in the Appendix).

3.5.3 German Microcensus

To discuss potential sample issues arising due to the lack of information on children's eligibility status in our baseline samples, we additionally draw upon the German Microcensus (GMC). Providing information on children's country-of-birth, their parents country of birth as well as their parents' year of arrival to Germany, the GMC allows us to define the group of second-generation immigrant children as well as their eligibility status. A comparison between the average second-generation migrant child – defined as children born in Germany, but to foreign-born parents – and the migrant child eligible for birthright citizenship – defined as as children born in Germany whose parents immigrated to Germany prior to 1992 – is shown in Table B.1 in the Appendix. Yet, given few information on children's educational outcomes – the GMC only contains children's secondary school track –, we only draw on the GMC for robustness analysis (see Section 6.4).

The only notable difference between children eligible for birthright citizenship and the average second-generation migrant child relates to their parents' country of origin. The arising pattern – disproportionally many children with Turkish ancestors qualify for

birthright citizenship, while disproportionally few children with ancestors from the former Soviet Union and the Balkans do so – aligns well with the different immigration waves and thus the necessary parental length-of-residence to qualify for birthright citizenship: larger immigration from Turkey occurred during the 1960ies, while immigration from the former Soviet Union and the Balkan occurred predominantly in the mid and late 1990ies.

3.6 Results

3.6.1 Preschool

Table 3.3 presents our results for the effects of automatic birthright citizenship at the preschool level. Column (1) only controls for the full set of month-of-birth dummies, while columns (2) to (3) sequentially condition on child and family background characteristics. In line with our assumption of random sorting around the cut-off, the estimates are robust to augmenting the set of control variables. In what follows, we discuss the results of our preferred specification controlling for the full set of background characteristics shown in column (3).

The estimates shown in Panel A show that the introduction of automatic birthright citizenship led to an 3.1 ppt increase in the preschool enrollment rate of immigrant children. Thus, the share of immigrant children not enrolled in preschool (8.3% among those born shortly before the cut-off date) decreased by 37%. Interestingly, this effect appears to have closed the preschool enrollment gap between native children – with an enrollment rate of 95.4% – and their immigrant peers completely.

Table 3.3: The effects of automatic birthright citizenship at the preschool level

	(1)	(2)	(3)
	A. Participation in the Early Education System		
Preschool Enrollment	0.034***	0.033***	0.031***
[0.917]	(0.012)	(0.011)	(0.010)
N	4,450	4,450	4,450
	B. Assessed Outcomes		
German Language Proficiency	0.029*	0.028*	0.035**
[0.580]	(0.015)	(0.014)	(0.013)
N	1248	1248	1248
Socio-Emotional Maturity	0.029***	0.021**	0.019*
[0.902]	(0.010)	(0.011)	(0.011)
N	3,441	3,441	3,441
	C. Recommendation		
School Readiness	0.024*	0.025	0.022
[0.863]	(0.014)	(0.016)	(0.017)
N	4,540	4,540	4,540
Birth Months	Yes	Yes	Yes
Child Characteristics		Yes	Yes
Family Characteristics			Yes

Notes: OLS estimates of equation (1) using the school entry examination (SEE) records 2005 and 2006. Mean of dependent variable for children born between July and December 1999 reported in square brackets. Child characteristics include gender and age at the SEE. Family characteristics include number of siblings, a dummy for single-parent household, parents' educational degree and parents' country of origin. Standard errors are clustered on a birthmonth/year level. * 10 percent, ** 5 percent, *** 1 percent confidence level.

Parental investments early in life together with preschool exposure are important inputs into children's human capital production function. In fact, it is well understood that preschool participation exerts positive effects on children's language, motor and social skills, particularly among children at risk for poor outcomes (see, for instance, Cornelissen *et al.* (2016) and Felfe and Lalive (2014)). In panel B, we therefore examine

the impact of birthright citizenship on immigrant children's proficiency in German as well as their socio-emotional maturity measured at the end of the preschool phase. The results suggest positive and significant effects on immigrant children's language proficiency: the share of immigrant children speaking fluently German rises from 58.0% to 61.5%. We also observe an improvement in immigrant children's socio-emotional development by 1.9 ppt – evidence in line with the results in Avitabile *et al.* (2014). Further inspection reveals that the improvement of children's socio-emotional maturity is mainly driven by a reduction in emotional problems, less so by a reduction in social deficits.

As part of the school entry examination, pediatricians provide an overall assessment of children's school readiness and may recommend deferred school entry. The results in panel C indicate that the reform had a positive, albeit no significant effect on pediatricians' recommendations regarding immigrant children's school readiness. This seems puzzling in light of the sizable positive effects on children's developmental indicators just discussed. One explanation for the lack of an effect might be that the positive findings for preschool enrollment and children's development were not driven by marginal children at risk of being identified as not ready for school. An alternative explanation might be that the rules governing the school readiness recommendation give rise to institutional discrimination against immigrant children, the incidence of which might have been unaffected by the reform. That said, and as mentioned earlier, the ultimate decision when to enroll a child for primary schools lies in the hands of parents, to which we now turn.

3.6.2 Primary School

Table 3.4 documents the effects of automatic birthright citizenship at the primary school level. Recall that our analysis here is based on the SR data and thus on estimation samples which are restricted to immigrant children who use a language different from German as their main language at home – a subsample which is likely dominated by children from disadvantaged backgrounds.

The key message that emerges from the first row in panel A is that the introduction of automatic birthright citizenship led to a reduction in immigrant children's school starting age by 1.6 months. As we can see in the second row in panel A, this effect is driven by an increased incidence of early school entry among immigrant children and not by a reduction

in children deferred from regular school entry: the share of immigrant children starting primary school at age 5 instead of age 6 increases by 9.9 ppt or to put it differently, more than doubles in comparison to the pre-reform mean.

Table 3.4: The effects of automatic birthright citizenship at the primary school level

	(1)	(2)
	A. Age At Primary School Start	
Age at School Entry	-1.562***	-1.571***
[76.906]	(0.327)	(0.329)
N	1,587	1,587
Early School Entry	0.099***	0.099***
[0.086]	(0.020)	(0.020)
N	1,611	1,611
	B. Educational Progress	
Grade Retention	-0.056*	-0.058**
[0.236]	(0.028)	(0.028)
N	1,576	1,576
Birth Months	Yes	Yes
Child Characteristics		Yes

Notes: OLS estimates of equation (1) using the school register (SR) data from 2009 and 2010. Mean of dependent variable for children born between July and December 1999 reported in square brackets. Child characteristics include gender. Family characteristics are not available for the SR data. Standard errors are clustered on a birthmonth/year level. * 10 percent, ** 5 percent, *** 1 percent confidence level.

The timing of primary school entry may have an effect on children's educational outcomes, although the direction of this effect is *a priori* unclear. On the one hand, being older at school entry comes along with being relatively more mature. If early relative maturity effects may propagate themselves over time, starting school younger may have an overall negative effect on children's educational outcomes (Bedard and Dhuey, 2006). Yet, starting school younger may be an advantage if children learn more in school than at home or in preschool environments. For immigrant children, with relatively high rates

of social and economic disadvantages among their families, an earlier integration into the school system may indeed be beneficial. Moreover, parents may be encouraged or even forced to get more involved in their children's education if children are enrolled earlier in school. Due to data limitations, we are not able provide a comprehensive analysis of children's educational outcomes during primary school. However, the data allow us to examine the effects of the reform on the probability of grade retention. Panel B shows a sizable reduction post-policy in the probability of grade retention among immigrant children. The point estimate from our preferred specification indicates a reduction in grade retention by 5.8 ppt or 24.6%. This effect reduces the difference in grade retention between immigrant children and their native counterparts by roughly one-half.[26]

3.6.3 Secondary School

In Table 3.5, we focus on the effects of birthright citizenship at the secondary school level. After four years of primary school, the German education system separates children into three educational tracks that differ in academic orientation: secondary lower school, intermediate school, and Gymnasium, the academic track. Before children are tracked into these differing-ability schools, primary school teachers make a recommendation which secondary school track a child should attend. The results in panel A show that the likelihood of immigrant children receiving a recommendation for the academic track of secondary school was not significantly affected by the reform – the estimate of our preferred specification shown in column (2) corresponds to an increase by 4.2 ppt or 26.4%, but lacks significance at any conventional level. While this finding is somewhat unexpected given the positive effects we have uncovered at the preschool and primary school level, it is in line with existing evidence that parental background, even conditional on student achievement, is a key explanatory factor for teachers' track recommendations (Lüdemann and Schwerdt, 2013). Such discriminatory behavior is unlikely to have been affected by the policy.

Recall, however, that in the state and period under study, the ultimate decision which track a child should follow in secondary school was at the discretion of parents. As such,

[26]Indeed, the grade retention rate among pre-reform native children (15.6%) was 11.5 ppt lower than that of pre-reform immigrant children (27.1%)

we may still observe an effect of the policy change on immigrant children's selection into different secondary school track. Panel B shows this to be, indeed, the case. In particular, the reform increased the share of immigrant children attending the academic track of secondary school by 9.5 ppt, or 49.0% of the pre-reform mean. The summary statistics in Table 3.2 suggest that the estimated effect reduces the academic-track enrollment gap between native and immigrant children by 38.0%. We next ask: is this effect driven by immigrant parents being more likely to comply with teachers' recommendations to enroll their children in Gymnasium; or are immigrant parents more likely to override teachers' recommendations, and send their children to Gymnasium despite the lack of an official recommendation? We find that both possibilities play a role. On the one hand, the proportion of immigrant children attending the high school track of secondary school *with* an official recommendation increases by 4.9 ppt or by 49.0 %. On the other hand, the share of immigrant children attending the academic track *without* an official recommendation rose by 4.5 ppt or 52.3% of the pre-reform mean. The latter result suggests that the policy induced immigrant parents to override the recommendations of teachers to enable their children access to higher education.

Table 3.5: The effects of automatic birthright citizenship at the secondary school level

	(1)	(2)
	A. Recommendation	
Recommendation for	0.042	0.042
Gymnasium [0.154]	(0.027)	(0.027)
N	1,299	1,299
	B. Track Choice	
Gymnasium	0.093***	0.095***
[0.194]	(0.027)	(0.027)
N	1,611	1,611
Gymnasium with	0.049**	0.049**
Recommendation [0.108]	(0.022)	(0.022)
N	1,611	1,611
Gymnasium w/o	0.045*	0.045**
Recommendation [0.086]	(0.022)	(0.022)
N	1,611	1,611
Birth Months	Yes	Yes
Child Characteristics		Yes

Notes: OLS estimates of equation (1) using the school register (SR) data from 2009 and 2010. Mean of dependent variable for children born between July and December 1999 reported in square brackets. Child characteristics include gender. Family characteristics are not available for the SR data. Standard errors are clustered on a birthmonth/year level. * 10 percent, ** 5 percent, *** 1 percent confidence level.

3.6.4 Sensitivity Analysis

Sample issues: representativeness, eligibility.— The results presented so far are based on administrative data which do not allow us to determine immigrant children's eligibility status. The SEE samples contain immigrant children who are not necessarily born in Germany and whose parents may not have resided long enough in Germany; the SR samples – while containing information on the country of birth – are restricted to

Table 3.6: Evidence from the German Microcensus

	(1)	(2)	(3)
Eligible Children: Gymnasium	0.047*	0.054*	0.059**
[0.246]	(0.027)	(0.028)	(0.025)
N	961	961	961
Birth Months	Yes	Yes	Yes
Child Characteristics		Yes	Yes
Family Characteristics			Yes

Notes: OLS estimates of equation (1) using the GMC waves 2010 and 2011. Mean of dependent variable for children born between July and December 1999 reported in square brackets. Child characteristics include gender and age quarter dummies. Family characteristics include number of siblings, a dummy for single-parent household, parents' educational degree and parents' country of origin. Standard errors are clustered on a birthmonth/year level. * 10 percent, ** 5 percent, *** 1 percent confidence level.
(Source: Research Data Center of the Statistical Offices of the Länder and the Federal State, Microcensus, 2010-2011, own calculations.)

immigrant children who use a language different from German at home. To address these issues, we provide results based on data from the GMC (see Table 3.6). To reiterate, the GMC has two advantages over administrative data. First, we can properly define the group of second-generation immigrant children based on information of their own country-of-birth as well as their parents country of birth. Second, the GMC contains the information necessary to construct children's eligibility status. Yet, as mentioned previously, the GMC allows us to analyze only one educational outcome, secondary school track choice. Finally, given the rather small sample size, 916 eligible second-generation immigrant children across the whole federal territory of Germany born between July 1998 and June 2000 and observed at the onset of secondary school, we can not distinguish between different federal states and thus between states with binding and non-binding teacher recommendation.

Table 3.6 shows that our results based on the SR data are not specific to immigrant children who do not speak German at home, but hold true for all second-generation immigrant children eligible for automatic birthright citizenship. Importantly, estimates barely vary when augmenting the set of control variables providing the balancing test we were

unable to conduct with the SR data. When controlling for the full set of individual background characteristics, the results suggest that automatic birthright citizenship led to an increase in immigrant children's enrollment in the academic track of secondary school by 5.9 ppt, or to put it differently to an increase by 24.0%. Interestingly, this point estimate lies in between our baseline estimates for the effect of automatic birthright citizenship on immigrant children's enrollment with recommendation and on immigrant children's overall enrollment rate – a result which is in line with the fact that the results based on the GMC data rely on both states with and without binding teacher recommendations.

Specification issues.— We also subject our results to robustness checks by repeating the analysis for the alternative specifications discussed in Section 3.4.3: (i) we implement the so-called donut strategy by dropping the months just around the cut-off date (i.e., December and January); (ii) we restrict our samples to children born before the ratification of the citizenship reform by narrowing the window around the cut-off date (from 12 to 8 months); (iii) we draw on one additional post-reform cohort and control flexibly for possible time trends by interacting being born between January and June with each birth cohort.

Table 3.7 presents the results. The following points are worth noting. The fact that our results are robust to specifications (1) and (2) indicates that patterns of selective timing of fertility are unlikely to be a concern in our setting. The estimates for immigrant children's participation in the early education system, primary school starting age and secondary school track choice remain unchanged no matter the sample restriction. This also holds true for the estimate related to immigrant children's enrollment in the academic track of secondary school without an official recommendation. The estimates related to teachers' recommendation regarding children's secondary school track choice, however, looses magnitude once restricting the sample to an 8-months window around the cut-off date – a result pointing towards increased parental investment being the driving force behind immigrant children's improved access to the highest educational track. Finally, estimates based on three cohorts and controlling flexibly for possible time trends – see column (3) in Table 8 – are also comparable in size and precision. This result

raises confidence that there are unlikely any further differences between children born in different semesters of a school year that could bias our results.

Table 3.7: Robustness

	(1) Donut	(2) 8-month window	(3) Trend
	A. Preschool		
Preschool enrollment	0.042***	0.033**	0.034***
	(0.012)	(0.013)	(0.013)
N	3,713	3,046	6,740
Language Proficiency	0.413*	0.037***	0.035**
	(0.239)	(0.010)	(0.016)
N	1,019	841	2,730
Socio-Emotional Maturity	0.007	0.021*	0.029**
	(0.011)	(0.013)	(0.012)
N	2,813	2,306	5,169
School Readiness	0.026	0.015	0.016
	(0.028)	(0.014)	(0.016)
N	3,713	3,046	6,740
	B. Primary School		
Age at School Entry	-1.925***	-1.145**	-1.577***
	(0.341)	(0.401)	(0.357)
N	1,323	1,028	2,498
Early School Entry	0.122***	0.063**	0.100***
	(0.019)	(0.023)	(0.022)
N	1,343	1,037	2,530
Grade Retention	-0.032	-0.070*	-0.056*
	(0.030)	(0.039)	(0.030)
N	1,313	1,019	2,482
	C. Secondary School		
Recommendation	0.077***	0.025	0.041
	(0.026)	(0.034)	(0.030)
N	1,073	832	1,959
Gymnasium	0.116***	0.085***	0.093***
	(0.030)	(0.030)	(0.030)
N	1,343	1,037	2,530
Gymnasium with Recommendation	0.075***	0.029	0.050**
	(0.022)	(0.021)	(0.024)
N	1,343	1,037	2,530
Gymnasium w/o Recommendation	0.041*	0.057*	0.044*
	(0.021)	(0.031)	(0.025)
N	1,343	1,037	2,530

Notes: OLS estimates of equation (1) using the school entry examinations (SEE) 2005 and 2006 and the school register (SR) 2009 and 2010, the estimates shown in specification (3) draw additionally on the SEE 2007 and the SR 2011. Control variables include age and gender. Standard errors are clustered at the birth month/year level. * 10 percent, ** 5 percent, *** 1 percent confidence level.

3.7 Conclusion

Successfully integrating children with migrational backgrounds into the education system is high on the policy agenda in many countries. Yet, surprisingly little is known about causal factors underlying immigrant children's educational integration. In this project, we have taken some steps to fill this void by evaluating whether a major citizenship reform in Germany – one that saw the introduction of automatic birthright citizenship – helps closing the immigrant-native gap in a series of education outcomes over the early life cycle. Theoretically, a link might be expected because granting citizenship at birth can be viewed as a positive shock to the long-run rate of return on investments in children's human capital. Yet, in practice the effect is far from being clear: while an initial increase in parental investments may stimulate children's development and thus provoke continuous parental as well as child efforts, a lack of an echo in official evaluations and success may discourage parents as well as children from maintaining their efforts and thus have no result on children's educational outcomes in the long-run. We address this question empirically and exploit a birth date cut-off determining whether a child became eligible for birthright citizenship or not. This allows us to overcome problems of endogeneity using a difference-in-differences approach: our treatment group comprises children born shortly before and shortly after the reform's cut-off date, while children from the preceding birth cohort are used as the control group.

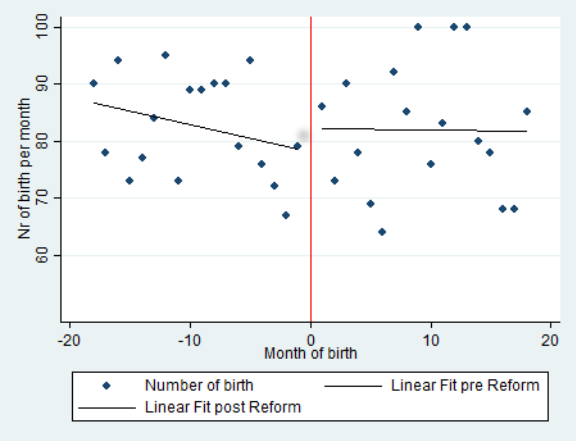
Our results show that the introduction of automatic birthright citizenship had sizable positive effects on the educational integration of immigrant children at the first three stages of the German education system. In particular, the policy (i) increased immigrant children's participation in non-compulsory preschool education; (ii) had positive effects on key developmental outcomes (e.g., German language proficiency) measured at the end of the preschool period; (iii) caused immigrant children to progress faster through primary school; and (iv) enabled them better access to the academic track of secondary school, a precondition for higher education and university studies. Our interpretation of these results is that the introduction of birthright citizenship has incentivized immigrant parents to provide their children with more similar educational opportunities as children in native families.

Our study offers some lessons for policy-makers and raises interesting questions for future research. Granting citizenship rights to immigrant children in places where they are poorly integrated into the education system might be an effective policy lever for reducing educational disparities. Indeed, when taken at face value, our results suggest that automatic birthright citizenship helps closing the immigrant-native gaps in key measures of educational success. While our analysis provides a range of insights, much remains to be done to understand the longer-run effects of birthright citizenship. The cohorts born around the German citizenship reform will soon enter the labor market, form their own families and participate in civic life. Understanding the long-run effects of birthright citizenship in these domains presents an important and rich agenda for future research.

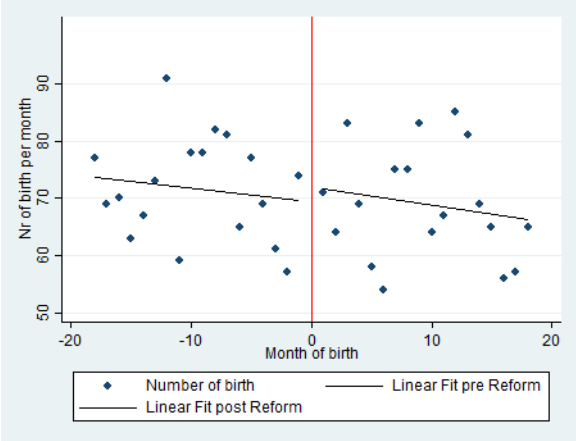
Appendix B

Figure B.1: Number of births by month of birth

(a) All immigrant children



(b) Eligible immigrant children



Source: Research Data Center of the Statistical Offices of the Länder and the Federal State, Microcensus, 2009-2012, own calculations.

Table B.1: Summary statistics: full v/s eligible sample – German Microcensus

	Migrant All Pre-reform	Migrant Eligible Pre-reform	Migrant Eligible II/99	Migrant Eligible I/00	Diff
		A. Dependent Variables			
Gymnasium	0.252	0.239	0.246	0.281	0.035
		B. Background Variables			
Age in Months	177.088	177.020	176.260	172.000	- 4.260***
Female	0.465	0.468	0.444	0.475	0.031
Nr. of Siblings	1.445	1.499	1.517	1.388	-0.129***
Single parent	0.173	0.129	0.125	0.116	-0.009
Mom's Education: low	0.333	0.362	0.336	0.343	0.007
Mom's Education: mid	0.240	0.218	0.250	0.244	-0.006
Mom's Education: high	0.135	0.108	0.121	0.128	0.007
Dad's Education: low	0.331	0.376	0.371	0.380	0.009
Dad's Education: mid	0.181	0.160	0.168	0.211	0.033
Dad's Education: high	0.108	0.096	0.103	0.136	0.033
Mom's origin: Turkey	0.305	0.431	0.392	0.369	-0.022
Mom's origin: Post-Soviet	0.201	0.086	0.108	0.128	0.020
Mom's origin: Balkan	0.089	0.054	0.086	0.087	0.001
Mom's origin: EU 12	0.103	0.136	0.125	0.120	-0.005
Mom's origin: other	0.189	0.179	0.190	0.161	0.029
Dad's origin: Turkey	0.273	0.409	0.384	0.355	-0.029
Dad's origin: Post-Soviet	0.177	0.076	0.091	0.120	0.029
Dad's origin: Balkan	0.076	0.050	0.082	0.091	0.009
Dad's origin: EU 12	0.067	0.093	0.086	0.099	0.013
Dad's origin: other	0.161	0.166	0.172	0.165	-0.007
Observations	1,219	719	232	242	

Notes: *Migrant All* refers to children born in Germany between June 1998 and December 1999 (i.e., pre-reform) and whose parents were both not born in Germany. *Migrant Eligible* restricts the group further to children whose parents fulfilled the residency criterion at the child's birth. *Migrant Eligible II/99* refers to eligible children born between July and December 1999. *Migrant Eligible I/00* refers to eligible children born between January and June 2000.
Source: Research Data Center of the Statistical Offices of the Länder and the Federal State, Microcensus, 2010-2011, own calculations.

Chapter 4

Citizenship, Identity, and Trust in a Migration Society: Combining a Large-Scale Behavioral Experiment with a Natural Experiment

This chapter is joint work with Christina Felfe from the University of St. Gallen, Martin Kocher from the University of Munich and the Institute of Advanced Studies, Vienna, Helmut Rainer from the ifo Institute and the University of Munich, and Thomas Siedler from the University of Hamburg.

4.1 Introduction

Immigration has shaped, and continues to shape, many rich nations. This brings with it the challenge of integrating immigrants and their children into society and labor markets. Among economists, the common take on integration—and on policies that promote it—is to emphasize convergence in the outcomes of immigrants and those of the host population in economic dimensions such as educational attainment and labor market participation.

Another fundamental, but much less scrutinized, aspect of integration pertains to non-market interactions between immigrants and natives. Many such interactions, from basic exchanges in everyday life to workplace cooperation to the provision of neighborhood amenities, are not governed by complete contracts and therefore require informal underpinnings such as a common understanding that a handshake is a handshake. When this is in doubt, mutual gains from exchange may not be realized due to distrust (Bowles, 2015). For this reason, it is often said that trust is the lubricant of social systems (Arrow, 1974)[27].

But does trust also tie together segmented migration societies? If a nation is fractured along lines of race, religion or country of origin, and if people only trust those with whom they share a common identity, then cooperation between social groups can break down, with consequences such as conflict, inefficient outcomes and high transaction costs (Basu, 2010). In a similar vein, the role of trust in generating efficient outcomes would also seem limited when social exclusion or lack of economic opportunity leads to the adoption of oppositional identities by those in disadvantaged groups (Akerlof and Kranton, 2000). Evidence that supports these lines of thought shows that ethnic cleavages in communities or societies at large are inimical to trust (Zak and Knack, 2001; Alesina and La Ferrara, 2002). Thus, to think clearly about integration interventions, we need to not only know about their impact in terms of convergence; we should also be concerned about their potential in building trust and cooperation between social groups.

We study the effects of a major citizenship reform in Germany—a change from the "law of blood" to the "law of soil"—in terms of social difference, trust and discrimination between immigrant and native youth. Our unique setup combines the advantages of

[27]The general importance of trust and social capital for economic development and growth has been highlighted again and again—one influential survey of the literature is provided by Fehr (2009).

experimental economics in studying in-group and out-group phenomena with the way in which labor economists have come to frame causal questions. In particular, we (i) conducted an artefactual field experiment (Harrison and List, 2004) based on the trust game with a large, non-selected sample (N=4,436) of German adolescents;[28] (ii) asked participants to condition their strategies on the migration background of their opponents; (iii) matched the experimental data with individual background information from an extensive, self-conducted socioeconomic survey; and (iv) chose a design that allows us to connect the artefactual field experiment with a natural experiment—i.e., the citizenship reform—using quasi-experimental strategies.

Three aspects of our approach deserve comment: the context of the study, the artefactual field experiment, and the citizenship reform to which we connect it.

In recent years, Germany has become the OECD's second largest country of immigration after the United States (OECD, 2017), and immigrant youth currently account for one-third of the nation's children under the age of 15. A first major dividing line between natives and immigrants in Germany is religion: the majority of native German children have a Christian religious affiliation, while Islam is the dominant religion among immigrant children. Next, the evidence from our own survey, as well as other household surveys in Germany, suggests that immigrant children are more likely than non-immigrant children to have parents with low educational attainment, to grow up under low-income conditions, and to experience little language assimilation within their families. Finally, a good deal of evidence also indicates that immigrant children are outperformed by their German peers along multiple indicators of academic achievement. Thus, we are dealing with native and immigrant children who are strongly segmented in terms of socioeconomic and cultural characteristics.

Our artefactual field experiment builds on the pioneering work of Fershtman and Gneezy (2001), who were among the first to use the trust game (Berg *et al.*, 1995) as a vehicle for measuring trust and discrimination therein between real social groups.[29]

[28] As will be explained in more detail below, we have avoided attrition in the experiment by running it in 219 classes of 57 German schools during regular school hours in the final year of compulsory schooling. We incentivized the trust game monetarily, and paid out all 4,436 participants privately and in cash.

[29] In another study based on a real-group design, Falk and Zehnder (2013) conducted a trust game experiment in the city of Zurich, in which first movers could condition their investments on the residential districts of second movers.

We take trust as measure for cooperation (or, efficiency of social interaction) within and between social groups, and the version of the trust game we implement is based on the following idea.[30] In a segmented migration society like the German, "being a native" or "being an immigrant" are amongst the core attributes of individuals (next to gender) that determine their social identity. Moreover, these attributes are ubiquitous and difficult not to perceive and therefore likely to feed into social interactions. Thus, we asked participants to condition their strategies on the identity of their opponents, classified as belonging to six possible social groups: (i) boys with German parents, (ii) girls with German parents, (iii) boys with foreign parents, (iv) girls with foreign parents, (v) naturalized boys with foreign parents, and (vi) naturalized girls with foreign parents. Our survey, in turn, allows us to determine which social group a participant belongs to, allowing us to study cross-group discrimination in trust behavior (henceforth, trust discrimination). To be clear, we use the term discrimination to describe the actions of players—in particular, differential trust decisions vis-a-vis in-groups and out-groups—but do not intend it to be a description of their underlying motives. These motives may involve statistical discrimination, wrong stereotyping or identity-based social preferences, and we will analyze how relevant these are in our context. Our experiment was run at the schoolclass-level in 57 German schools, and each of our 219 sessions was either preceded or succeeded by a one-hour slot in which we administered an extensive socioeconomic survey to our participants.

The citizenship reform to which we connect the experiment was implemented on January 1, 2000 and saw the introduction of *birthright citizenship*. This new regime based on *jus soli* (right of soil) replaced the principle of *jus sanguinis* (right of blood) under which German citizenship could only be acquired by descent from a German legal mother and/or a German legal father. The nature of the reform, and the way we exploit it empirically, allows us to causally analyze whether it changed the behavior of immigrants towards natives but not *vice versa*.[31] Our motivating hypothesis was as follows. In theory,

[30] We do not attempt to provide a preference or motivational anatomy of trust, but are, of course, aware of the fact that decisions to trust can involve different motivations such altruism, risk attitudes *et cetera* (see, e.g., Fehr (2009)).

[31] Naturally, the behavior of natives towards immigrants may also depend on whether the latter are naturalized or not. We investigate this possibility through our experiment but not by exploiting the citizenship reform.

socioeconomic differences between social groups may prevent efficient social interactions between the members of these groups (Lazear, 1999). Now, through the reform, a significant portion of immigrant children automatically acquired German nationality by birth, and thus the same legal rights—and as such political and professional opportunities—as their native counterparts. In Chapter 3 a subset of us argues that this increased the returns to education for immigrants, and found indeed evidence that the policy substantially reduced a pronounced, pre-existing immigrant-native gap in education. Thus, we held the expectation that the introduction of birthright citizenship may have also had a positive effect on the efficiency of interactions between immigrant children and their native peers in social dilemma situations. We test this hypothesis among the first cohort of immigrant children affected by the reform, more than 15 years after it took effect. With regard to empirics, the reform provides us with a birth date eligibility cut-off, which serves as our source of exogenous variation.

We present four main sets of results. First, we document an asymmetric pattern of trust and discrimination therein: immigrant children (i.e., those with foreign parents) show a high willingness to trust those with whom they share a migration background, which, however, does not extend to natives (i.e. children with German parents). For example, one of our results shows that immigrant children transfer (on average) 60% of their initial endowments to other immigrants, while their transfers to native German children are 13% lower. Native children, by contrast, are much less inclined to condition their trust decisions on the identity of their opponents: they transfer 58% of their initial endowment to other natives, but their out-group investments to immigrants are only 2% lower. Looking at these results in terms of propensities to discriminate, they imply that roughly one in three immigrant children can be classified as strong discriminators (in-group investment/out-group investment>1.25), while the corresponding share for native children is only half as large. The data also reveal that immigrant girls discriminate more against natives than immigrant boys. Native girls, by contrast, show no signs of discrimination against immigrants, while native boys discriminate moderately.

Second, we seek to provide an explanation for the observed patterns of trust discrimination. We first check for statistical discrimination in our experiment. The idea is that the discriminatory trust decisions of children with migrational backgrounds can

be considered as statistically rational if they receive lower back transfers from native children than from other immigrants. Our results show this not to be the case. We next check for discrimination based on wrong stereotypes. After the first stage of the trust game, we asked participants to indicate their expectations about the back payments of the six possible receiver types. Looking at these expectation data, we find that immigrant children expect natives to show a higher willingness to reciprocate than their own peers, suggesting that their discriminatory trust decisions are not due to mistaken stereotypes. This then points to preference-based discrimination—that is, different trust norms for in-groups and out-groups—as the likely explanation for immigrants' differential trust decisions. We furthermore show that immigrants' discriminatory actions impose a pecuniary externality on native children and involve monetary losses for themselves. This provides an analogy to economic models of identity, in which the behavior of individuals adopting oppositional identities can be "anti-social" and "self-destructive". We therefore interpret our results as reflecting an oppositional culture of trust.

Third, we recognize that frictions in social interactions might be related to how culturally distant or proximate different social groups are. In our data, we detect a strong correlation between religious background and the extent to which different immigrant groups discriminate against natives: we observe above-average levels of trust discrimination for immigrant groups with Muslim backgrounds and below-average levels of trust discrimination for immigrant groups that share with the native majority a Christian religious background. In fact, the most-pronounced above-average discriminatory trust choices are made by immigrant children of Turkish origin—the by far largest minority of youths in Germany and almost exclusively of Muslim background.

As our main contribution, we finally turn to the question of whether governments can use citizenship law to create and nurture mutual trust between immigrants and natives. As mentioned above, the introduction of birthright citizenship in Germany provides us with a birth date eligibility cut-off determining whether children born to foreign nationals were automatically granted the German nationality at birth or not. This birth date cut-off serves as our source of identification. In particular, we use a difference-in-differences design which not only compares the experimental choices of immigrant children born shortly before and shortly after the cut-off date, but also draws upon

native German children—who were unaffected by the birth date cut-off—as a control group. Our main finding is that the policy substantially reduced the degree of trust discrimination among male, but not among female, immigrants. We furthermore show that this effect is particularly pronounced for immigrant children of Turkish origin, i.e., the largest group of immigrant youth in Germany. Several robustness checks corroborate this result. Last, we provide evidence suggesting that the main channel for this effect is an improved educational integration of immigrant males.

To date, there is an impressive body of experimental research on discrimination in economics and psychology (for an insightful review up until the mid-2000s, see Anderson *et al.* (2006)). Notably, many of these studies induce group membership in the laboratory. In contrast, we deliberately ran a lab-in-the-field experiment with immigrant and non-immigrant children, and allowed participants to condition their choices on the migration background of possible opponents. To our knowledge, this type of design was originally implemented by Fershtman and Gneezy (2001) to study trust and discrimination in the segmented Israeli society. In another study closely related to our own, Falk and Zehnder (2013) conducted a trust game experiment in the city of Zurich, in which first movers could condition their investments on the residential districts of second movers.

Our study adds several unique aspects to the existing literature. To the best of our knowledge, we are the first to combine a large-scale artefactual field experiment with a natural policy experiment. Delivering such a connection and determining the causal effect of a particular integration policy on experimentally-elicited discriminatory behavior is one of our key contributions. A second unique aspect of our study is that we provide experimental data for a large, non-selected sample of adolescents. In particular, we have avoided selection effects in the artefactual field experiment by conducting it during regular school hours in the last year of compulsory schooling. For the 57 participating schools, our sample comprises almost the universe of all adolescents in a particular age cohort.

In terms of policy implications, we believe that our results suggest a need to rethink conventional wisdom about migrant integration. Discrimination and prejudice *against* immigrants is an issue that figures prominently in many public and scholarly debates. This largely ignores the oppositional culture of trust that we have identified, i.e, that immigrants' trust appears, to an extent, confined to those with whom they share a

migration background. The consequences of this include potentially large reductions in social welfare, especially if one recognizes that greater trust and cooperativeness in a society is like a public good. On the positive side, the oppositional trust choices of immigrant children are not set in stone. In particular, immigrant children who—through the introduction of birthright citizenship—were raised to the same legal status as their native counterparts appear to have extended their innate sense of trust to their native peers. Thus, an important takeaway message is that governments can modify and nurture social values, in our case resulting in greater trust between strongly segmented social groups.

The remainder of the chapter is structured as follows. Section 4.2 describes our experimental design and provides descriptive statistics from our survey. Section 4.3 presents results, and Section 4.4 contains our conclusions.

4.2 Experiment and Survey

Our experiment took place between June and November 2015, and was conducted in 219 classes (=sessions) of 57 German schools during regular school hours. At the time of the experiment, all participants were in their final year of compulsory schooling.

4.2.1 Setting, Subject Pool, and Sample Description

The experiment was run in two German federal states, each characterized by an independent education system: Schleswig-Holstein (SH), where the duration of compulsory schooling is 9 years; and North Rhine-Westphalia (NRW), where compulsory education lasts for 10 years. In both federal states, a school year starts in August/September and ends in June/July. There were two waves of data collection. In the first wave lasting from June 2 to July 16, our target population were all 9th graders of 31 schools (spread over 122 classes) in six cities of SH.[32] In the second wave lasting from October 19 to November 16, we targeted all 10th graders of 26 schools (spread over 97 classes) in two cities of NRW.[33] Our chosen target populations allow us to focus on a school cohort comprised

[32]The cities are Flensburg, Kiel, Lübeck, Neumünster, Elmshorn, and Pinneberg, with population sizes ranging from 42,266 in Pinneberg to 246,306 in Kiel.

[33]The cities are Duisburg and Wuppertal, with population sizes of 491,231 and 350,046, respectively.

of children mainly born in 1999 and 2000—thus 15 to 16 years of age at the time of the experiment. This, in turn, will allow to study a major German citizenship reform, which took affect on January 1, 2000 and saw the introduction of birthright citizenship.

In both SH and NRW, we sought approval for our study by submitting the design and a list of target schools to the respective ministries of education. Both ministries approved the study and encouraged the principals of the targeted schools to participate in it. Upon this, we contacted the principals themselves and asked for formal permission to conduct the experiment and survey in all classes of grade 9 in SH and in all classes of grade 10 in NRW, respectively. The participating schools belong two five school types: 10 schools are secondary general schools ("Hauptschule"); 8 are intermediate schools ("Realschule"); 29 are comprehensive schools without the final years of grammar school-type education ("Gesamtschule ohne gymnasialer Oberstufe"); 8 are comprehensive schools with the final years of grammar school-type education ("Gesamtschule mit gymnasialer Oberstufe"); and 2 are grammar schools ("Gymnasium"). Two weeks prior to the study, school principals informed parents about the study, who were given an opt-out option, i.e., they could proscribe their children's participation. Moreover, immediately before the experiment started, all students present in class were informed by us that participation is voluntary. The experiment was run at the schoolclass-level during two regular consecutive school hours.[34]

At the day of the experiment, a total of 4,634 students were present in the 219 classes. Parents made use of the opt-out option for 44 of them (less than 1%), while 154 students (3.5%) chose to opt out themselves. Thus, 4,436 students participated in the study. Of those, 133 participants did not fully complete the experimental task, while 226 did not provide the survey information necessary for our basic analysis (i.e., on own gender and/or parental migration background). This leaves us with a baseline sample of 4,077 students.

Each of our 219 sessions was either preceded or succeeded by a 45-minute slot in which we administered an extensive socioeconomic survey to the participants. The survey includes a total of 55 questions, and provides information, *inter alia*, about participants' personal background, school achievements, aspirations, preferences and interests, and

[34] A school hour in Germany lasts 45 minutes.

family background. Key participant variables include date of birth, place of birth, and gender. Two key family background variables are the birth places of both parents, which we use to categorize participants into three groups: (i) native children, whose parents are both German-born; (ii) immigrant children, whose parents are both foreign-born; and (iii) mixed-background children, who have one German-born and one foreign-born parent. Overall, our sample comprises 2,201 native children (54%), 1,218 immigrant children (30%) and 658 mixed-background children (16%). Roughly 77% of all immigrant children in our sample are German-born (i.e., second-generation immigrants), while 23% are foreign-born (i.e. first-generation immigrants).

Through the bulk of our analysis we will focus on native and immigrant children, while discussing results for mixed-background children only in passing. Thus, the now following sample description will be confined to the former two groups (for details, see Appendix Table D.1). In Germany, the by far largest minority of youths are immigrant children of Turkish origin. This is also evident in our sample. Specifically, 38% of immigrant children have parents who are Turkey-born, 14% have Middle-Eastern or African backgrounds, 12% have parents born in a post-Soviet country, 11% have parents from a Balkan country, 10% have Eastern European backgrounds, and 14% stem from other countries. A comparison of native and immigrant children suggests several marked differences, of which we mention four. First, roughly one-third of immigrant children have parents with low educational attainment, while the corresponding share for native children is just under one-fourth.[35] Second, immigrant children are more likely than non-immigrant children to live in two-parent households (74% vs. 55%). Third, the majority of native children report a Christian (i.e., Catholic or Protestant) religious affiliation (67%), while the group of immigrants is predominantly made up of Muslim children (59%). Finally, we observe a low degree of language assimilation in migrant families, with 69% of immigrant children reporting that they speak a language other than German with their parents. We interpret this evidence as reflecting the pronounced cultural, social and economic gaps between native and immigrant children that we also observe in representative surveys of German households.

[35] As Appendix Table D.1 also shows, a relatively large portion of immigrant children report that they don't know their parents' educational attainment.

4.2.2 The Trust Game

Our experiment is a modified version of the standard trust game (Berg *et al.*, 1995), which consists of two players called the first-mover (sender) and the second-mover (receiver). We endowed all players with five euros at the beginning of the game. The first-mover decides whether to send any money to the second-mover, and if so, how much. The transferred amount of money (x) is then tripled by the experimenter. The second-mover is informed about the first-mover's decision and the transferred amount ($3x$) and can then decide whether to send back any amount $y \in [0, 5+3x]$ to the first-mover. The final payoff for the first-mover is $5 - x + y$, and for the second-mover it is $5 + 3x - y$.

In our trust experiment, we employed the strategy method, i.e., each participant had to play the trust game both as first-mover and as second-mover. Moreover, and most importantly for the purpose of this paper, we allowed first-movers to condition their investment decisions on the gender and migration background of possible interaction partners. We implemented this by letting first-movers decide whether, and if so, how much to transfer to six possible receiver types (indexed by k): a boy with German-born parents (S_1), a girl with German-born parents (S_2), a boy with foreign-born parents (S_3), a girl with foreign-born parents (S_4), a naturalized boy with foreign-born parents (S_5), and a naturalized girl with foreign-born parents (S_6). In principal, this setup allows us to understand the extend to which trust is dependent on opponents' migration background as well as gender. However, the main task that this paper sets itself is to trace whether there is trust discrimination based on migration background. To achieve it in the simplest possible way, we will largely abstract away from trust conditional on gender, apart from sporadic remarks when deemed necessary. Thus, we collapse the six choices $\{S_1, ..., S_6\}$ into two variables: a participant's average investment to natives (S_N) and his or her average investment to immigrants (S_I), defined as

$$S_N = \frac{1}{2}\sum_{k=1}^{2} S_k \quad \text{and} \quad S_I = \frac{1}{4}\sum_{k=3}^{6} S_k.$$[36]

[36]It should be noted that receiver types $k \in \{3, 4\}$ capture immigrants generally (i.e., boys and girls with foreign-born parents), while receiver types $k \in \{5, 6\}$ only the subset of naturalized immigrants (i.e., naturalized boys and girls with foreign-born parents). The reason why we have allowed for this distinction will become clear in Section 4.3.4, where we examine the effects of the German citizenship reform. For our main results, we have chosen not to drop any data and hence compute S_I by averaging of their

Throughout the paper, we will refer to S_N as native children's in-group investments and as immigrant children's out-group investments, respectively. Likewise, we will refer to S_I as native children's out-group investments and immigrant children's in-group investment, respectively. Our main outcome measure of interest, which we shall call *trust discrimination* (see, e.g., Falk and Zehnder, 2013), is the in-group/out-group investment gap of children with and without migrational backgrounds. Formally, it is defined as

$$TD = \begin{cases} S_N - S_I & \text{for native children;} \\ S_I - S_N & \text{for immigrant children.} \end{cases}$$

After participants had completed the investment stage of the trust game, they were asked to indicate on their decision sheet the expected back payment from the six possible interaction partners.

At the final stage of the trust game, participants were asked to play the role of second-movers, and we employed the contingent response method to elicit their back payments. For example, on a first decisions sheet, participants were asked to decide on their back transfers to a boy with German-born parents. Specifically, for each of the eleven possible investments of the first-mover (i.e., a boy with German-born parents), the second-mover had to decide whether and how much to send back. Using the same strategy vector variant, we elicited back payments to the other five potential interaction partners.

4.2.2.1 Implementation

The experiment was conducted as a paper-and-pen experiment. It lasted approximately one school hour (45 minutes). We ensured anonymity by assigning a unique identity code to each participant. Before the experiment started, the instructions were distributed to all students in class and read out by an experimenter.[37] To guarantee privacy, we installed

investments to receiver types $k \in \{3, 4, 5, 6\}$. That said, our results do not hinge on this specifications and we will present robustness checks where we compute S_I by averaging over participants' investments to receiver types $k \in \{3, 4\}$, i.e., by letting $S_I = \frac{1}{2}(S_3 + S_4)$.

[37]The German instructions can be found in Appendix C. All the study sessions were run by either one of the authors or a student assistant, who was prior trained by the authors. The experiments were conducted by one leading conductor and one or two assistants. We randomized the sequence of survey and experiment on a daily basis in order to avoid any potential bias stemming from that sequence.

mobile privacy screens between students.[38] Students were informed that they would first play the trust game as first-movers and thereafter as a second-mover. They were told that they can earn real money and that their payoffs would depend on their own choices and those of another, randomly assigned experiment participant from a different school.[39] The average payoff in the experiment was 7.26 €. Participants received their payoffs no later than two weeks after the experiment took place (in envelopes with their identity codes, distributed by school secretaries or head teachers), which was know to them at the beginning of the experiment. All participants faced exactly the same decision tasks, instructions, and payoffs.

4.3 Results

The material in this section is divided into two main parts. The first (Sections 4.3.1 to 4.3.3) analyses how children with and without migrational backgrounds interact in our trust game, while the second (Section 4.3.4) connects the artefactual field experiment with the natural citizenship experiment.

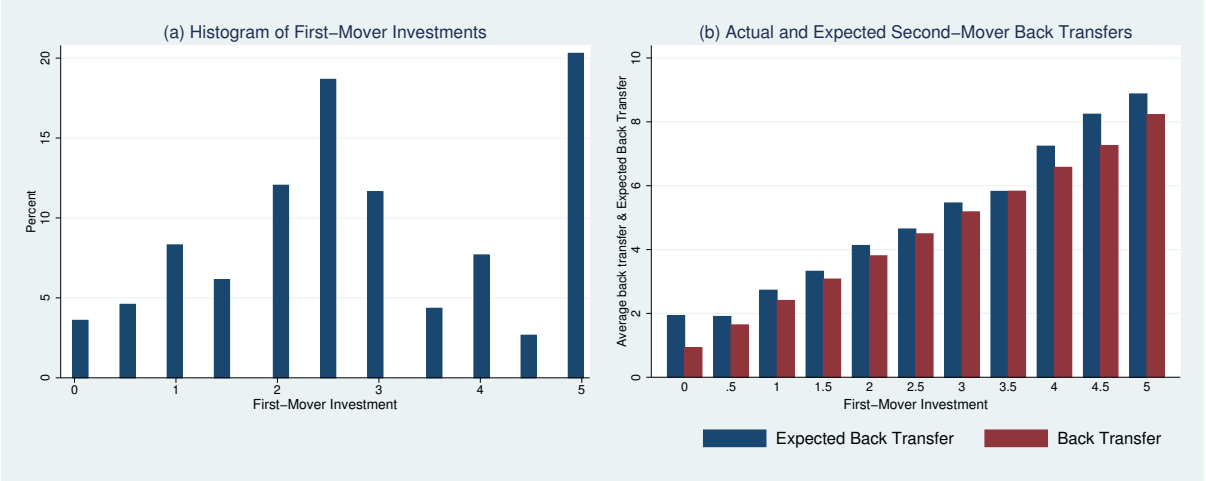
4.3.1 General Patterns of Trust and Reciprocity

We begin with a brief description of general patterns of trust and reciprocity in our experiment. Panel (a) in Figure 4.1 shows a histogram of all investment decisions in the experiment. On average, first movers invest 2,85 € or 57% of their initial endowment. The two most frequent investment choices are transfers of respectively 50 and 100% of the initial endowment. These investment patterns are comparable to what has been observed in laboratory experiments.

[38] See picture C.1 in Appendix C.

[39] To be precise, participants were informed that we would calculate final payoffs as follows: (i) we randomly match two participants from two different schools; (ii) we randomly assign the roles of first- and second-mover; (iii) we determine the true type k of both the first- and second-mover based on survey information on own gender and whether parents are German-born or foreign-born; (iv) we implement the first-mover's decision for the true type of the second-mover; (v) we implement the second-movers back payment for the true type of the first-mover and his or her choice implemented in step (iv); and (vi) based on the pair of choices implemented in steps (iv) and (v), we calculate the participants' final payoffs.

Figure 4.1: Trust, reciprocity and expectations



Notes: Panel (a) shows a histogram of *all* investment decisions in the experiment. Since we used the strategy method to collect the decisions, each subject made 6 investment decisions, one for each of the possible groups of the second movers. All these decisions are included in the data underlying this figure. Panel (b) shows actual and expected second-mover back transfers. Red bars show averages of second mover transfers for each possible first mover investment. Note that each second mover indicated a transfer decision for each possible first mover investment. The data underlying this figure thus contains eleven decisions per second mover. Blue bars show means of expected back transfers conditional on own first mover investments.

Panel (b) in Figure 4.1 shows (i) averages of second mover back transfers for each possible first mover investment and (ii) average expected back transfers conditional on own first-mover investment. It is apparent that second movers show reciprocal behavior: the higher first mover investments, the higher are back transfers. The degree of reciprocity appears to be quite high. For example, second movers are willing to send back about 4.50 € if they receive 2.50 € , which almost equalizes final payments. Over the whole range of investments, the ratio of pay backs to investments estimated from an OLS-regression is 1.42.

Finally, Panel (b) also reveals that, on average, the level of actual back transfers matches first-movers' expectations about back transfers almost one-to-one, especially for intermediate investments.

4.3.2 Who Discriminates against Whom?

We now examine the prevalence of trust discrimination among children with and without migrational backgrounds. For the main part of the analysis, we restrict our sample to native German children (i.e., both parents German-born) and their immigrant peers (i.e.,

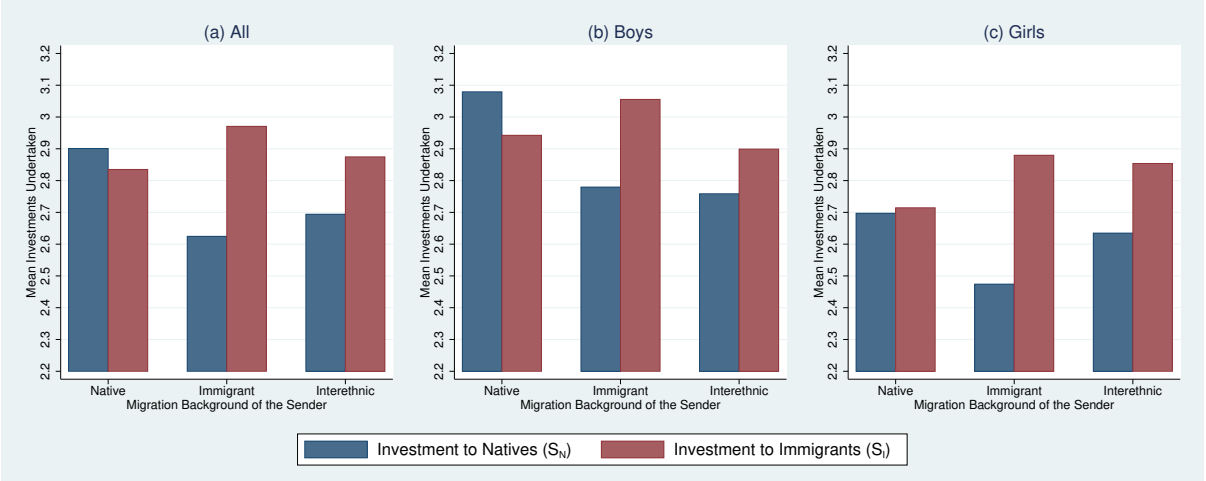
both parents foreign-born). At the end of the section, we briefly discuss the experimental choices of children with one native and one foreign-born parent.

4.3.2.1 Trust Decisions

The first question we focus on is whether the migration background of game partners affects the trust decisions of first movers. To that end, we first exploit for each participant the average investment to natives (S_N) and immigrants (S_I), respectively. Figure 4.2 illustrates the group-conditioned investment decisions of native and immigrant children, both for the entire sample and separately by gender. There are several interesting takeaway messages here. First, for native German children, the evidence speaks against a strong pattern of mistrust toward immigrant children. In the full sample, natives' in-group investments exceed their out-group investments by a statistically significant 2.1% ($S_N = 2.90, S_I = 2.84$; paired t-test with $p < 0.00$). Looking at this result separately by gender, we observe that there is no in-group/out-group variation in the trust decisions of immigrant girls ($S_N = 2.70, S_I = 2.72$). Native boys, by contrast, reveal a moderate degree of trust discrimination: their in-group investments exceed their out-group investments by a statistically significant 4.8% ($S_N = 3.08, S_I = 2.94$; $p < 0.00$). Second, among immigrant children, we detect a strong mistrust toward natives which manifests itself in a statistically significant in-group/out-group investment gap of 13.4 percent ($S_I = 2.97, S_N = 2.62$, $p < 0.00$). The subgroup results by gender suggest that this investment gap is more pronounced for immigrant girls (16.1%; $p < 0.00$) than for immigrant boys (10.1%; $p < 0.00$).[40]

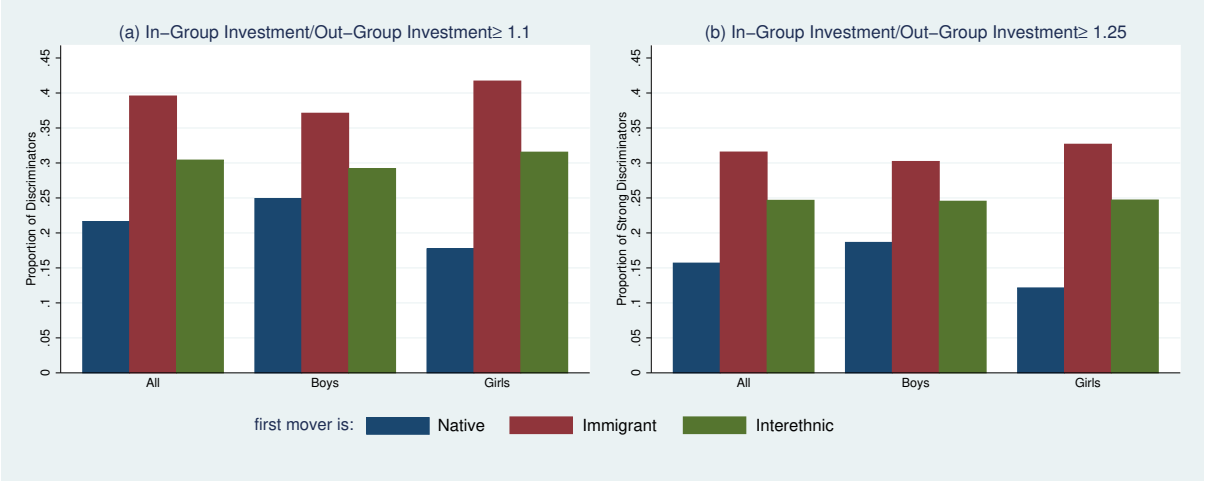
[40]In Figure E.1 we show how the migration background *and* gender of game partners affects the trust decisions of first movers. A general finding is that first movers investment more of their initial endowment when the second mover is a girl rather than a boy. Moreover, this result does not depend on the migration background of first movers and that of their game partners. Specifically, both children with and without migration backgrounds appear to positively and in almost equal measure discriminate between native girls and native boys and immigrant girls and immigrant boys, respectively.

Figure 4.2: Investments of native and immigrant children by migration background of second movers



In Figure 4.3, we look at trust decisions in terms of propensities to discriminate between in-group and out-group opponents. To do so, we classify participants as *discriminators* (respectively, *strong discriminators*) if their in-group investments exceed their out-group investments by 10% or more (respectively, 25% or more). The results are quite striking. Panel (a) suggests that about one in five native German children make discriminatory trust decisions (21.6%), while the corresponding share is almost twice as large for immigrant children (39.7%). A two-sample t-test reveals that the difference is significant ($p < .00$). Next, it is interesting to point toward the apparent gender-specific patterns of trust discrimination: the immigrant-native gap in the propensity to discriminate is more pronounced among girls (41.7 v/s 17.7%) than among boys (37.1 v/s 24.9%)—a result mainly driven by the fact that native girls display a fairly low propensity to discriminate. Panel (b) illustrates the incidence of *strong* discrimination. It can been seen that roughly one in three immigrant children (31.6%) strongly discriminate against second movers of German origin. By contrast, the share of strong discriminators among native children is only half as large (15.7%).

Figure 4.3: Proportions of (strong) discriminators



We now turn attention to our main dependent variable of interest: *trust discrimination* (TD), which is defined for each first mover as the difference between average in-group investment and average out-group investment. Compared to the propensities to discriminate discussed above, this variable thus captures the full variation in discriminatory behavior. Consider the following simple regression model:

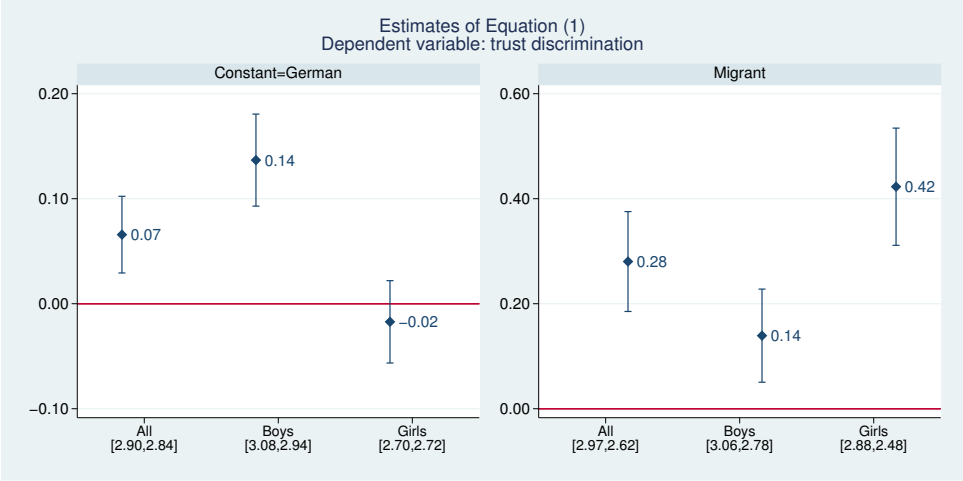
$$\text{TD} = \alpha_0 + \alpha_1 \text{Migrant} + \varepsilon. \tag{4.1}$$

In this specification, the omitted category are native children, i.e., those whose parents are both German-born. *Migrant* is a binary variable indicating whether a child has parents who are both foreign-born. Standard errors are clustered by school programme and location.

Figure 4.4 presents unconditional OLS estimates of equation (4.1). The key message here is that the pattern of trust discrimination that we identified above is statistically highly significant. In the full sample (labeled "All" on the x-axis), the mean of trust discrimination is 0.07 € for native children, while for immigrants it is a statistically significant 0.28 € higher. Among boys, the in-group/out-group investment gap for natives mounts to 0.14 €, while for their immigrant peers it is exactly twice as high, with the difference being significant at the 1% level. For native girls, the mean of trust discrimination is both quantitatively and statistically indistinguishable from zero (-0.02 €), while

the average level of trust discrimination for immigrant girls is a statistically significant 0.42€ higher.

Figure 4.4: Trust discrimination



Notes: Unconditional OLS estimates of Equation (1). Standard errors clustered by school programme and location. Whiskers indicate the 95% confidence interval. In square brackets, we report means of average in-group investments (first entry) and average out-group investments (second entry), respectively. Sample Sizes: 3,419 (All); 1,789 (Boys); 1,630 (Girls).

4.3.2.2 Which Types of Discrimination Can Explain Observed Behavior?

After discussing who discriminates against whom, we now examine which types of discrimination can explain the in-group/out-group patterns we identified in the trust game. In particular, we first exploit children's second mover decisions to determine whether there is statistical discrimination. Second, to shed light on the role of discrimination based on stereotypes, we ask whether children's trust decisions vis-a-vis players of different migrational backgrounds are mirrored in differential expectations of reciprocity. To economize on space, we provide an in-depth discussion of our results in Appendix A and summarize the main findings here.

Immigrant Children.—In their seminal paper on trust in the segmented Israeli society, Fershtman and Gneezy (2001) pointed out that ethnic trust discrimination may be rational when a comparison of different ethnic groups reveals systematic statistical differences with respect to reciprocal behavior. In a similar vein, in our study, the discriminatory trust decisions of children with migrational backgrounds could be considered as justified on statistical grounds if they receive lower back transfers from native children than from

other immigrants. We find this not to be the case. In particular, the average (of the 11 contingent) back transfers of native children to immigrant first mover types (4.43 €) are almost identical to the average back transfers of immigrant children to immigrant first mover types (4.47 €), with the difference being statistically indistinguishable from zero and the pattern not being dependent on the gender of immigrant first movers (see Appendix Figures F.1 and F.2). We conclude, therefore, that the discriminatory trust decisions of immigrants are not a reflection of statistical discrimination.

Although immigrant children do not receive lower back transfers from natives than from immigrants, they may have the wrong perception that they do. If this were the case, the trust decisions of immigrants vis-a-vis natives could be interpreted as discrimination based on wrong stereotypes. In our experiment, first movers indicated on their decision sheet the expected back transfer from the six possible receiver types. We find that, conditional on own investments as first movers, immigrant children do not systematically expect that they will receive lower back payments from native than from immigrant receiver types. This result holds for both immigrant boys and girls (see Appendix Figures F.3 and F.4). We therefore conclude that discrimination based on wrong stereotypes does not drive the differential trust decisions of immigrants. Since our results on repayment behavior also speak against statistical discrimination, this points to preference-based discrimination—that is, different trust norms for in-groups and out-groups—as the likely explanation for immigrants' differential trust decisions. In terms of social efficiency, immigrants' discriminatory actions impose a pecuniary externality on native children and involve monetary losses for themselves (see Appendix Table F.1). This then provides a link to economic models of identity, in which the equilibrium choices of individuals adopting oppositional identities can be "anti-social" and "self-destructive". We therefore view the discriminatory trust decisions of immigrants as reflecting an oppositional culture of trust.

Native Children.—Applying the same analysis to children without migrational background, we find that the average (of the 11 contingent) back transfers of native children to native first mover types (4.51 €) exceed the average back transfers of immigrant children to native first mover types (4.31 €) by a statistically significant 4.6%. This result applies equally if we consider the average back transfers of native and immigrant children to

native boys and girls, respectively (see Appendix Figures F.1 and F.2). Thus, a possible interpretation is that the first mover in-group/out-group investment gap of 4.8% among native boys (see Figure 4.2 above) reflects discrimination against immigrants on statistical grounds. An analysis of our expectations data supports this interpretation, showing that native boys have the (correct) expectation that they will receive higher back transfers from native than from immigrant children (see Appendix Figure F.4). The final open question then relates to the trust decisions of native girls: although they receive lower back transfers from immigrants than from natives, they do not discriminate against immigrants as first movers. Our expectations data suggest that this could be due to wrong, positive stereotypes whereby they misjudge the behavior of immigrants as second movers. In particular, native girls do not reveal a substantial expectation differential with respect to back transfers from immigrants and natives, respectively.

4.3.3 Do Different Immigrant Groups Discriminate the Same Way?

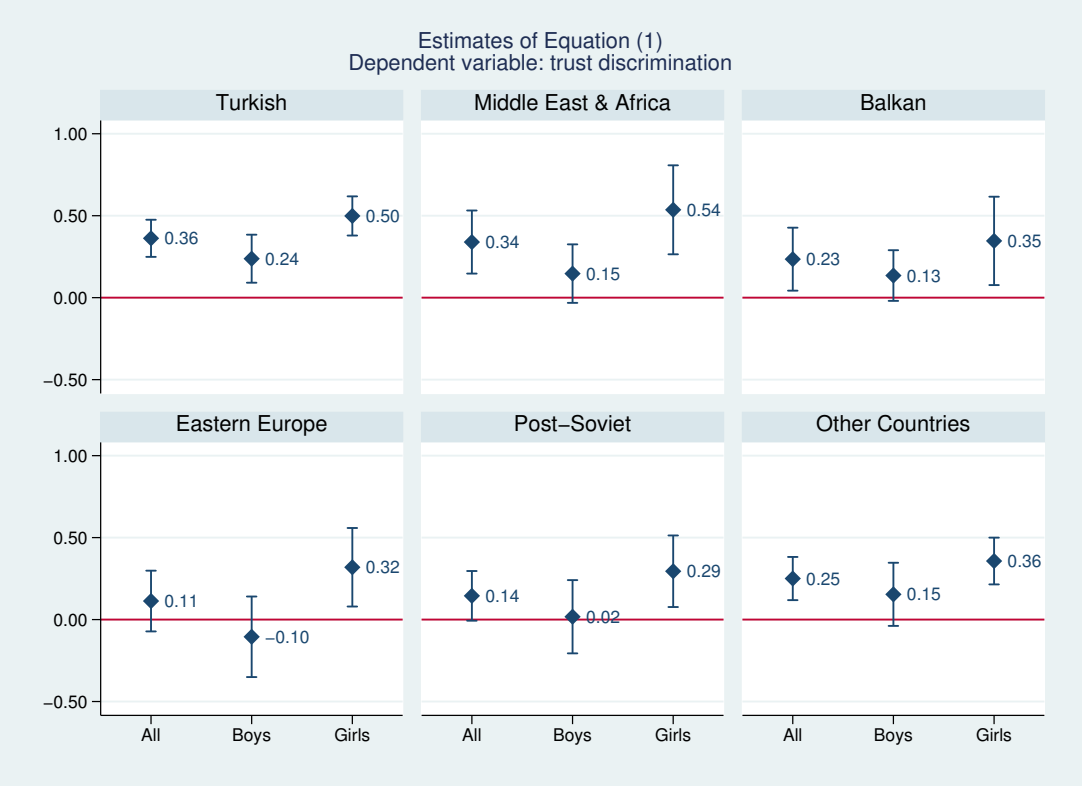
Intuition would suggest that immigrant children's discriminatory trust decisions are related to how culturally distant or proximate they are to their native peers. In the German context, religion is one potentially important marker of cultural similarity between children with and without migrational backgrounds, with different immigrant groups displaying a high degree of heterogeneity in this respect.

We now once more run Equation (1), but with the variable *Migrant* replaced by dummy variables for six mutually exclusive groups of immigrant children with varying religious backgrounds. The first group (*Turkey*; 461 observations) comprises immigrant children of Turkish origin—the by far largest minority of youths in Germany. In the second group (*Middle East & Africa*; 280 observations) we pool together immigrant children with Middle-Eastern and African backgrounds. The third group (*Balkan*, 137 observations) is made up of second-generation immigrant children whose parents come from a Balkan country. The fourth group contains immigrant children with an Eastern European background (*Eastern Europe*; 130 observations), while the fourth group is made up of immigrant children whose parents come from a post-Soviet-country (*Post-Soviet*; 130 ob-

servations). The final, miscellaneous group (*Other Countries*, 176 observations) contains all other immigrant children. The first three immigrant groups share the characteristic that they are predominantly made up of children with a Muslim background (Turkey: 92%, Middle East & Africa: 75%, Balkan: 68%). By contrast, the religious background of immigrant children with an Eastern European or post-Soviet background—with shares of Christians of 84% and 61%, respectively—is comparable to that of native German children (67%). The final group comprises immigrant children with mixed religious backgrounds (share of Muslims: 39%, share of Christians: 31%).

Figure 4.5 shows that the most pronounced above-average discriminatory trust decisions are made by immigrant children of Turkish origin. In particular, while the in-group/out-group investment gap for German native children amounts to 0.07 € (Boys: 0.14 €; Girls: -0.02 €), it is 0.37 € (Boys: 0.24 €; Girls: 0.50 €) higher for children with Turkish-born parents. The second group of immigrants that displays an above-average level of trust discrimination are children with Middle Eastern and African backgrounds. By contrast, immigrant children whose parents come from Eastern European and Post-Soviet countries reveal a below-average level of trust discrimination. Noticeable in this respect is that the in-group/out-group investment gap of boys in these two immigrant groups is identical or even lower than that of German native boys. F-tests on equality of coefficients on the dummies *Turkey* and *Eastern European* reject equality at p-values of 0.006 (full sample), 0.02 (boys) and 0.11 (girls). A comparison of the coefficients on the dummies *Turkey* and *Post-Soviet* yields a qualitatively similar conclusion.

Figure 4.5: Trust discrimination, by ethnic background



Notes: Unconditional OLS estimates of Equation (1), with *Migrant* replaced by 6 dummy variables indicating whether a child's mother was born in Turkey, an Eastern European or post-Soviet country, a Balkan country, a country of the Middle East, an African country, or a country other than those. Standard errors clustered by school programme and location. The omitted category are native German children. Estimates of the constant (=trust discrimination among native children; non-reported) correspond to those reported in Figure 4.4 (All: 0.07, Boys: 0.14, Girls: -0.02). Whiskers indicate the 95% confidence interval. Sample Sizes: 3,419 (All); 1,789 (Boys); 1,630 (Girls).

Two final points should be made about the analysis so far. First, we have focused on comparing the experimental choices of children with and without migrational backgrounds, but have been silent on the behavior of children with mixed-backgrounds (658 observations). Appendix Figure G.1 reveals that mixed-background children also discriminate in favor of immigrants and against natives ($S_I = 2.88$; $S_N = 2.69$; gap 7.1%; $p < .00$), but to a lesser extent than immigrant children and with less pronounced gender differences. Second, in discussing the choices of children with migrational backgrounds, we have made no distinction between first-generation (i.e., foreign-born) and second-generation (i.e., German-born) immigrant children. Our empirical strategy in the next section requires us to narrow our sample to second-generation immigrant children. Ap-

pendix Figure G.3 shows that the extent of discrimination that we identified in the trust game does not differ markedly between first- and second-generation immigrant children.

4.3.4 Birthright Citizenship and Trust Discrimination

This section turns to the final question posed at the outset: can governments of immigrant-receiving countries influence the interface between trust and social identity through integration measures? We address this question by analyzing how one important instrument of integration policy—that is, access to the host-country nationality—affects children's experimentally-elicited trust decisions.

4.3.4.1 Pathways to Citizenship for Immigrant Children: *Jus Soli* vs. *Jus Sanguinis*

The path to citizenship for immigrant children varies considerably across immigrant-receiving countries. In the United States, any person born on the nation's territory automatically gains U.S. citizenship, regardless of the nationality or immigration status of the person's parents. This rule based on *jus soli* ("right of soil") has been in place since the 19th century and is commonly referred to as birthright citizenship. By contrast, many countries in Europe have historically granted citizenship at birth based upon the principle of *jus sanguinis* ("right of blood"), meaning that citizenship is inherited through parents rather than determined by the place of birth. For children born to foreign nationals, this rule implies that citizenship can only be acquired through naturalization (i.e., upon application) later in life. Not surprisingly, in countries which have *jus soli* virtually all native-born children of immigrants have the host-country nationality, while the lowest percentages of children of immigrants with host-country nationality are found in countries that adhere to *jus sanguinis* (OECD, 2011). The context of our study is Germany, a country which has recently witnessed a switch from *jus sanguinis* to *jus soli*. This switch provides a unique opportunity to study the effects of birthright citizenship on immigrant children's behavior.

4.3.4.2 A Natural Experiment: The Introduction of Birthright Citizenship in Germany

Throughout the 20th century, German citizenship could not be acquired through birth on German territory but by descent from a German mother and/or a German father. With the turn of the millennium, this principle of *jus sanguinis* was replaced by a conditional version of *jus soli*. In particular, a child born to foreign parents after December 31, 1999 automatically acquired German nationality at birth if at that time one of its parents had been living in Germany habitually and legally for at least eight years and had permanent residency rights. Official German citizenship data show that among the 91,273 children born to foreign nationals in Germany in the year 2000, 41,257 were automatically granted citizenship at birth (Federal Office for Migration and Refugees, 2012). This suggests that roughly 45% of all immigrant children born in the first post-policy year were eligible for automatic birthright citizenship. For immigrant children with parents of Turkish origin—the by far largest minority in Germany—the share of eligible immigrant children amounted to 78%. In contrast, children born before January 1, 2000 could only become German citizens later in life if at least one of the parents chose to naturalize him or herself.[41] This setting allows for an analysis of the effects of a policy change from restricted citizenship for immigrant children (i.e., the old *jus sanguinis*) to automatic birthright citizenship (i.e., the new *jus soli*).

4.3.4.3 Possible Effects of Birthright Citizenship

There are (at least) two channels through which the introduction of birthright citizenship could have influenced the discrimination that we identified in the trust game. The first is what we shall call social identity change. The acquisition of host-country nationality entitles immigrants to full citizen rights in spheres such as politics, employment, welfare or health care, and is consequently seen by many as promoting their identification with the host country. Thus, one might hypothesize that the reform reduced the social distance between immigrants and their native peers, which in turn may have affected immigrants' discriminatory trust decisions in two ways: *directly* by influencing the degree to which

[41] Briefly describe transition rule here; state that the reform did not increase parents willingness to naturalize; refer to our previous paper.

they themselves differentiate between in-groupers and out-groupers; and *indirectly*, if immigrant children are treated differentially by their native peers based on their citizenship status. A second candidate channel to consider is educational integration. As discussed earlier, immigrant children in Germany are lagging behind in school performance when compared to their native counterparts. In the previous Chapter 3 a subset of us argued that the introduction of birthright citizenship in Germany increased the returns to education for immigrants,[42] and found indeed evidence for a positive human capital effect. In particular, the policy caused immigrant parents to send their children to preschool more often, to enroll them earlier in primary school, and to enable them access to the academic track of secondary school. This reform effect has reduced the distance between immigrant children and their native peers in the sphere of education, which might give rise to a different degree of differentiation between in-groupers and out-groupers.

4.3.4.4 Empirical Strategy

We consider the introduction of birthright citizenship on January 1, 2000 as an exogenously timed event that substantially facilitated access to host-country nationality for immigrant children. To isolate the causal effect the reform had on immigrant children's behavior, we compare the experimental decisions of *second-generation* immigrant children (i.e., German-born children with foreign-born parents) born before and after January 1, 2000. A potential concern with this comparison is that children born after the policy change are always younger than those born before. Moreover, it is possible that season of birth effects influence the composition of parents over the year. If age or season of birth has an impact on immigrant children's behavior, any potential reform effect will be biased. To net out these potential biases, we therefore construct a second difference between pre-policy and post-policy native German children (i.e., German-born children with German-born parents) to estimate a difference-in-difference (DiD) effect of birthright citizenship on experimentally-elicited behavior.

[42]There are several reasons for this. For example, citizenship improves immigrant children's future professional opportunities by enabling them access to employment in the public sector. Moreover, employers frequently face lower administrative costs if they wish to employ a naturalized person rather than a foreigner. Naturalization might also function as a signaling device for the employer for better integration, which in turn may influence immigrants' bargaining power. Last, it is conceivable that discrimination reduces degree of discrimination against children with a migration background from the side of the teachers.

In our DiD approach, it is important to ensure that immigrant parents could not self-select into treatment. Since our source of identification is a birth date cut-off, the main concern is strategic fertility behavior. We address this issue in two ways. First, we restrict our sample to children born in the ±4-month window around January 1, 2000. This ensures that our sample only comprises children who were conceived before July 1999, the month in which the German citizenship reform was ratified and announced. The ±4-month window has the additional advantage that it maintains the comparability of pre-policy and post-policy children with respect to educational progress. In particular, it excludes pre-policy children who (i) had to repeat a grade or (ii) entered school after their scheduled year of admission (i.e., "late school starters").[43] Moreover, it excludes post-policy children who entered school prior to their scheduled year of admission (i.e., "early school starters"). Second, we implement a "donut" DiD that drops children born in the ±2-week window around January 1, 2000. This avoids potential selection into treatment through birth-date-manipulation by parents.

Data limitations do not allow us to restrict the sample to eligible children, namely those born to *foreign nationals* who fulfilled the *residency criterium* ($\geq$ 8 years) when the reform came into effect.[44] We instead restrict the treatment group to second-generation immigrant children, i.e., those whose parents are both foreign-born. Thus, the DiD estimates capture the intention-to-treat effect (ITT) of access to the German nationality at birth. This ITT effect is a lower bound of the average treatment effect on the treated (ATT) since it is estimated on a sample which includes (i) pre-policy children who qualified for citizenship at birth through *jus sanguinis* and (ii) post-policy children who were ineligible for birthright citizenship due to their parents' residency status.

Throughout this section, we use two samples: a broad sample (BS) in which the control group comprises native children and the treatment group is made up of *all* second-generation immigrant children; and a narrow sample (NS) in which we restrict the treatment group to second-generation immigrant children with a Turkish background. As mentioned above, children with a Turkish background are the by far largest minority of

[43] In Germany, children usually enter school in June/July after turning six years old.

[44] Explain that our survey contains a question on parents' length of residence in Germany, but that this measure is prone to measurement errors. If results are ok, refer to a possible robustness checks that narrows the sample towards eligibles.

children in Germany, and we use them as a separate treatment group because they share a homogeneous cultural background and were more strongly affected by the introduction of birthright citizenship than the average immigrant child. In particular, for our two estimation samples, official German citizenship statistics (Federal Office for Migration and Refugees, 2012) and data from the German Microcensus suggest the following treatment intensities around the reform cut-off. In the broad estimation sample, roughly 45% of all post-policy second-generation immigrant children were eligible for automatic birthright citizenship, while 14% of all pre-policy second-generation immigrant children qualified for citizenship at birth through *jus sanguinis*. In the subsample of second-generation immigrant children with a Turkish background, the treatment intensity was substantially higher: 78% of all post-policy children where eligible for the new *jus soli*, while 14% of pre-policy children qualified for citizenship at birth through *jus sanguinis*. Thus, we will think of immigrant children with a Turkish background as a high-eligibility treatment group.

Taken together, the reform had a substantial impact on whether immigrant children possessed the German nationality from infancy onwards or not. However, it also had longer-term consequences for children's citizenship status which are still visible today: in our survey, administered 15 years after the policy change, 88.7% of all post-policy immigrant children in the broad sample report to possess the German citizenship, while the corresponding share for pre-policy immigrant children is only 71.7%. For immigrant children with a Turkish background, this difference still amounts to 33 percentage points (pre-policy: 62.7%; post-policy: 94.6%).

The DiD model we estimate is:

$$\begin{aligned} \text{TD} =& \beta_0 + \beta_1 \text{Migrant} + \beta_2 \text{Born Post-Reform} + \beta_3 (\text{Migrant} \times \text{Born Post-Reform}) \\ &+ \theta \text{Birth Month} + \xi \text{Family} + \vartheta \text{Classroom} + \varepsilon, \end{aligned} \tag{4.2}$$

where TD is our experimental measure for trust discrimination. *Migrant* is a binary variable indicating whether a child is a second-generation immigrant (=1) or a native (=0). The parameter β_1 captures differences between immigrant and native children born prior to the policy change. *Born Post-Reform* is a binary assignment variable indicating whether a child was born in the months just after January 1, 2000 (i.e., it is equal to one

for children born between January and April 2000, and zero for children born between September and December 1999). The coefficient β_2 measures general differences between children born before and after the birthright citizenship reform that could cause changes in trust discrimination even in absence of a policy change. The ITT coefficient of interest, β_3, multiplies the interaction *Migrant*×*Born post-reform*, thus identifying all immigrant children born after the policy change. To net out possible age and season of birth effects, we include a set of *Birth Month* dummies in all regressions. In extended specifications, we also include *Family* characteristics (i.e., maternal age, maternal education, family structure), and *Classroom* characteristics (i.e., class size, proportion of students with migration background, the gender ratio and five victimization measures[45]).

Our broad (respectively, narrow) estimation sample comprises 920 native German children and 360 second-generation immigrant children (respectively, 158 immigrant children with a Turkish background).[46] In order to verify whether treatment was balanced on observables, we present the mean values of key family and classroom characteristics for native and immigrant children and for children born before and born after the birthright citizenship reform in Appendix Tables H.1 and H.2. Importantly, the evidence shows that there are no considerable, systematic differences between children born before and after January 1, 2000. Among the 46 mean differences tests in both samples (see p-values in the last column of Tables H.1 and H.2, respectively), only four mean differences are statistically different from zero at the 5 percent significance level[47]. This supports the notion that the German citizenship reform was an "as-good-as-random" event with no systematic self-selection of particular types of immigrant families across the cut-off date.

4.3.4.5 Correlational Evidence

We first provide descriptive evidence on the behavior of second-generation immigrant children born around the reform's cut-off date. To do so, we restrict attention to immigrant

[45]These victimization measures capture the proportions of students who report having been victims, in the past year, of physical abuse, verbal abuse, lies, theft and exclusion

[46]We exclude children with incomplete information on parental migrant status and those with missing or inconsistent information about own country of birth.

[47]Two of the significant differences refer to mother's age, a difference that is to be expected given the nature of our reform threshold.

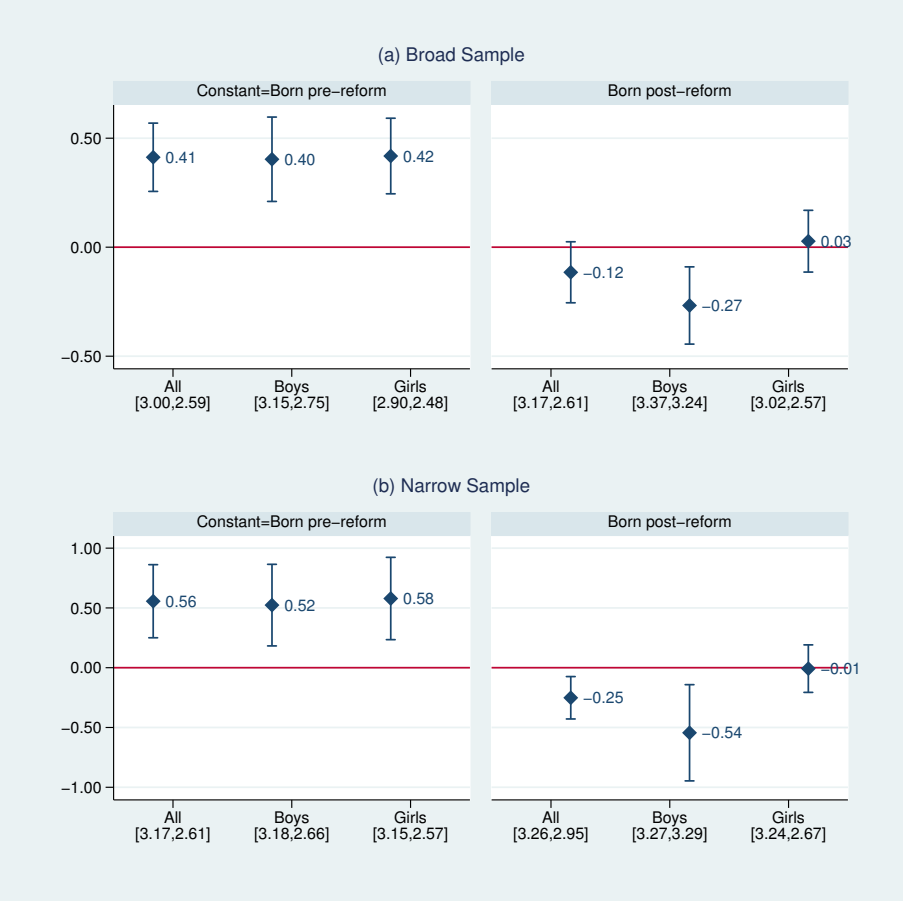
children and estimate the following (unconditional) simple regression model:

$$\text{TD} = \alpha_0 + \alpha_1 \text{Born Post-Reform} + \varepsilon$$

The dependent variable of main interest is *Trust Discrimination*. The explanatory variable *Born Post-Reform* is a binary variable indicating whether a child was born in the months before (=0) or after (=1) January 1, 2000. The sample is restricted to a 8-month window centered around this cut-off date and excludes children born in the 4-week window around it. Estimates of the parameter α_0 thus capture the behavior of children born pre-reform, while estimates of α_1 show how the behavior of children born post-reform differs from those born pre-reform.

Panel (a) of Figure 4.6 illustrates the results for second-generation immigrant children from our broad sample, revealing interesting gender-specific patterns. Let us first consider the behavior of boys: among pre-reform immigrant boys, investments to immigrants (3.15 €) exceed investments to natives (2.75 €) by 0.40 € or 15%. By contrast, among post-reform immigrant boys, the level of trust discrimination is 0.13 € and therefore a statistically significant 68% or 0.27 € lower, i.e., discriminating behavior almost vanishes for boys affected by the introduction of birthright citizenship. Turning now to the behavior of immigrant girls, it is interesting to observe that the birth date cut-off appears not to matter at all: among pre-reform immigrant girls, investments to immigrants exceed investments to natives by 0.42 € or 17%, and this level of trust discrimination persists for post-reform immigrant females.

Figure 4.6: Trust discrimination among immigrants born around the cut-off date



Notes: Sample comprises all immigrant children born between September 1999 and April 2000. 4-week donut around the cut-off. Standard errors clustered by school programme and location. In square brackets, we report mean investments to immigrants (first entry) and mean investments to natives (second entry), respectively. Whiskers indicate the 95% confidence interval.

Panel (b) of Figure 4.6 displays the point estimates for immigrant children with a Turkish background from our narrow sample. On average, pre-reform Turkish boys send 3.18 € to immigrants and 2.66 € to natives, respectively. The difference of 0.52 € corresponds to a (raw) trust differential of nearly 20%. Strikingly, this differential decreases by a statistically significant 0.54 € for post-reform Turkish boys. Put differently, on average, Turkish boys born under *jus soli* no longer discriminate between immigrants and natives in the trust game. In stark contrast, and consistent with the findings in Panel (a), we do not find evidence for a reduction in trust discrimination among immigrant Turkish girls. In sum, the descriptive evidence in Figure 4.6 suggests that the introduction of *jus soli* substantially reduced trust discrimination among immigrants and that this reduction is an entirely male phenomenon.

4.3.4.6 Causal Analysis

We now investigate whether the results presented so far can be given a causal interpretation. Table 4.1 presents estimates for several variants of equation (2), both for the broad sample (columns (1)-(3)) and the narrow sample (columns (4)-(6)). In each Panel (A-C), the estimated coefficients in the first row ($\hat{\beta}_1$) capture differences in trust discrimination between second-generation immigrant children and native children born prior to the policy change. The coefficient of interest is $\hat{\beta}_3$, which identifies the intention-to-treat effect of the citizenship reform on the extent of trust discrimination among immigrant children.

We start by discussing the results of regressions run for boys and girls together (see Panel A). In column (1), we estimate Equation (2) for the broad sample and condition only on gender and a full set of birth month fixed effects. For children born pre-policy, the immigrant-native gap in trust discrimination amounts to 0.36 €. The DiD estimate of -0.103 suggests that the introduction of birthright citizenship reduced this difference by about 29%, although the estimate is not statistically significant at conventional levels. Columns (2) and (3) show the result to be robust to including controls for family background and classroom characteristics, respectively. In Columns (4) to (6), we repeat the exercise for the narrow sample. Since immigrant children with a Turkish background were more strongly affected by the introduction of *jus soli* than the average immigrant child, we would expect a more pronounced reform effect. This is confirmed by all three specifications. For example, the DiD estimate of -0.254 from our preferred specification in Column (6) suggests that the policy reduced the pre-reform immigrant-native difference in trust discrimination (=0.497 €) by 51%. Moreover, in all three specifications based on the narrow sample, the reform effect gains statistical significance.

The remaining two panels of Table 4.1 break down the DiD estimates by gender. Panel B presents the results for boys. Consider first the estimated coefficients for the broad sample. Throughout all specifications, the reform effect turns out negative, large in magnitude and statistically significant at 5% level. In our preferred specification [Column (3)], the immigrant-native gap in trust discrimination among boys born pre-policy amounts to 0.281 €, but the introduction of *jus soli* reduced this difference by 0.26 € or 93%. In the narrow sample, this effect is even more pronounced and becomes statistically significant at the 1% level. For example, the DiD estimates from our preferred specifi-

cation [Column (6)] show a pre-reform immigrant-native gap in trust discrimination of 0.459 € and a policy-induced reduction thereof of 0.565 €. This suggests that the reform induced Turkish boys to discriminate *less* between in- and out-groupers than their native peers. In Panel C, where we repeat the analysis for immigrant girls, we find confirmation for the correlational evidence presented above: the effect of birthright citizenship on trust discrimination is an entirely male phenomenon. Specifically, both in the broad and the narrow sample, and irrespective of the specification, the DiD coefficients for immigrant girls are small in magnitude—both in absolute terms and relative to estimates of $\hat{\beta}_1$—and statistically indistinguishable from zero.

Table 4.1: Birthright citizenship and trust discrimination: diff-in-diff analysis

Dependent Variable: Trust Discrimination	Broad Sample			Narrow Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All						
Migrant ($\hat{\beta}_1$)	0.360***	0.343***	0.337***	0.505***	0.494***	0.497***
	(0.084)	(0.067)	(0.054)	(0.154)	(0.138)	(0.126)
Born post-reform*Migrant ($\hat{\beta}_3$)	-0.103	-0.099	-0.106	-0.245**	-0.244**	-0.254**
	(0.078)	(0.073)	(0.071)	(0.085)	(0.086)	(0.089)
Observations	1,280	1,280	1,280	1,078	1,078	1,078
R-squared	0.038	0.048	0.056	0.045	0.054	0.060
Panel B: Boys						
Migrant ($\hat{\beta}_1$)	0.278**	0.314***	0.281**	0.402**	0.460***	0.459***
	(0.109)	(0.100)	(0.112)	(0.157)	(0.153)	(0.157)
Born post-reform*Migrant ($\hat{\beta}_3$)	-0.261**	-0.284**	-0.260**	-0.558***	-0.571***	-0.565***
	(0.106)	(0.106)	(0.094)	(0.157)	(0.146)	(0.150)
Observations	618	618	618	529	529	529
R-squared	0.017	0.049	0.062	0.026	0.056	0.070
Panel C: Girls						
Pre-Reform Difference	*0.434****	*0.427****	*0.418****	*0.589****	*0.610****	*0.586****
Migrant ($\hat{\beta}_1$)	0.434***	0.427***	0.418***	0.589***	0.610***	0.586***
	(0.083)	(0.085)	(0.066)	(0.176)	(0.176)	(0.156)
Born post-reform*Migrant ($\hat{\beta}_3$)	0.045	0.037	0.034	0.0128	-0.060	-0.056
	(0.085)	(0.095)	(0.090)	(0.143)	(0.170)	(0.172)
Observations	662	662	662	549	549	549
R-squared	0.086	0.107	0.118	0.104	0.134	0.142
Month of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Family Characteristics		Yes	Yes		Yes	Yes
Class Characteristics			Yes			Yes

Notes: OLS estimates of Equation (2). Pre-reform difference refers to the *conditional* pre-reform difference in trust discrimination between treatment and control group. Standard errors clustered by school programme and location and reported in parentheses; p-values reported in square brackets. All specification in Panel A control for gender. *Family characteristics* include mothers' age, dummy variables for mothers' education (eight groups) and dummy variables for family structure (five groups) . *Class characteristics* include class size, proportion of students with migration background, the gender ratio and five victimization measures (i.e., the proportion of students who report having been victims, in the past year, of physical abuse, verbal abuse, lies, theft and exclusion). *** (**) (*) indicates significance at the 1% (5%) (10%) level.

Overall, we conclude that the introduction of birthright citizenship substantially reduced the degree of trust discrimination among immigrant boys, but had no measurable impact on the behavior of immigrant girls. Moreover, we observe above-average reform

effects for boys with a Turkish background, our high-eligibility treatment group for whom we have documented an above-average level of trust discrimination.

4.3.4.7 Robustness

We now probe the robustness of our main findings, both in the broad and the narrow sample. All sensitivity checks, which are reported in Table C.1 and Table C.2, are conducted for our preferred specification (see Table 4.1, columns (3) and (6), respectively).

For our first robustness check, we recalculate our main dependent variable, trust discrimination (TD), by letting $S_N = \frac{1}{2}(S_1 + S_2)$ and $S_I = \frac{1}{2}(S_3 + S_4)$ (see our discussion in Footnote 8). Columns (BS.1) and (NS.1) in Table C.1 show that the DiD estimates based on this alternative outcome measure remain qualitatively unchanged compared to benchmark results in Table 4.1.

In the next two robustness checks, we use the dichotomous outcome measures *Discriminator* and *Strong Discriminator* as dependent variables [see Columns (BS.2) to (NS.3)].[48] In a nutshell, the results suggest that the citizenship reform substantially reduced the share of (strong) discriminators among immigrant boys, especially in the narrow sample. For example, in Column (NS.3) of Panel B, we observe that Turkish boys born pre-policy are 20.4 percentage points more likely to be strong discriminators than their native counterparts, but the statistically significant reform effect of -20.8 percentage points eliminates this difference completely. By contrast, both in the broad and in the narrow sample, immigrant girls' propensity to (strongly) discriminate was unaffected by the introduction of *jus soli*.

In our DiD analysis, standard errors are clustered by school program and location, and there is a total of 18 clusters. Since reliable inference is a concern when there are few clusters (Donald and Lang, 2007; Cameron *et al.*, 2008), our fourth robustness check probes whether the results also hold using wilder cluster bootstrap t-procedures instead of clustering standard errors.[49] The estimates in Columns (BS.4) and (NS.4) show that

[48] Recall that we have defined participants as *discriminator* (respectively, *strong discriminator*) if their in-group investment exceed their out-group investment by 10% (respectively, 25%) or more.

[49] We estimated the wild cluster bootstrap standard errors using 1000 replications under H_1 as discussed in Cameron *et al.* (2008).

the p-values obtained from this alternative bootstrap procedure support the levels of statistical significance reported in Table 4.1.

The citizenship reform we study was ratified and announced in July 1999, but was already openly discussed in the German parliament during the previous month. Thus, our fifth robustness check provides DiD estimations with a narrower ±3-month window around January 1, 2000.[50] This additional restriction reduces the sample size by around 25%. Notwithstanding this, the results in Columns (BS.5) and (NS.5) show that the coefficients on the DiD interaction term remain largely unchanged compared to benchmark estimates in Table 4.1, although they are somewhat less precisely estimated.

Our sixth and final robustness check involves placebo reform regressions. In particular, we virtually shift the introduction of *jus soli* forward in time, assuming that it took effect on November 1, 1999. Moreover, we exclude all children born on or after January 1, 2000 from our placebo sample. The results in Columns (BS.6) and (NS.6) show that the coefficients on the DiD interaction term are close to zero (or even positive) and statistically indistinguishable from zero. We conclude, therefore, that the immigrant-native gap in trust discrimination did not converge in the pre-policy period.

4.3.5 Possible Explanations for the Reform Effect

In this section, we seek to provide possible explanations for the reform effect. In so doing, we also address the question of why it is gender-specific.

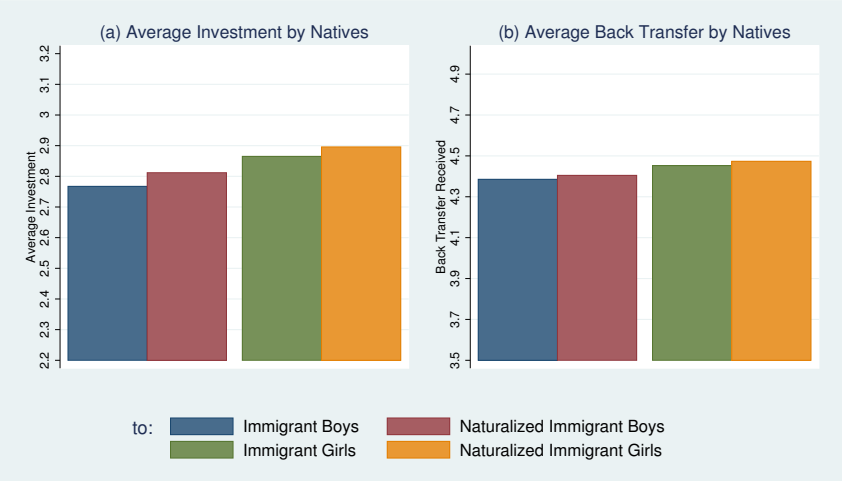
4.3.5.1 Differential Treatment by Natives?

The reduction in trust discrimination due to *jus soli* could be interpreted as a rational response by immigrant children if their native peers treat them differentially based on their citizenship status. Recall that, in our design, opponent types $k \in \{3, 4\}$ refer to immigrants as a whole (i.e., boys and girls with foreign parents), while opponent types $k \in \{5, 6\}$ to the subset of naturalized immigrants (i.e., naturalized boys and girls with foreign parents). In Figure 4.7, we analyze whether this distinction matters for the behavior of native children. In brief, the evidence suggests that native children do

[50] In line with our main specification, we continue to exclude children born in the ±2-week time window around the cut-off date

not systematically treat immigrants differentially based on their citizenship status. For example, in Panel (a), in which we illustrate the investment choices of native children as first movers, we observe a small bias in favor of naturalized immigrants, but the depicted investment gaps to immigrants as a whole hardly exceed 1%. In Panel (b), where we look at the back transfers of native children as second movers, the citizenship status of immigrant children appears not to matter at all. Finally, no gender-specific patterns can be observed. Thus, based on these findings, we conclude that differential treatment by natives is unlikely to be a main channel for the reform effect and its gender-specific nature.

Figure 4.7: Investments and back transfers of native children by gender and citizenship status of immigrant opponents



4.3.5.2 Educational Integration?

Evidence suggests that having the host country's citizenship has a positive impact on the labour market outcomes of immigrants: it allows them to earn higher wages (Chiswick, 1978; Steinhardt, 2012), to find jobs more easily (Fougère and Safi, 2009; Gathmann and Keller, 2017), and to steepen their wage-tenure profiles (Bratsberg *et al.*, 2002). Thus, by increasing the long-run rate of return to education, the introduction of *jus soli* in Germany may have induced immigrant parents to invest more in their children's human capital. If this in turn decreases the degree of educational segmentation between immigrants and natives, it may also affect inter-group interactions. Thus, as a first possible channel,

we now examine whether the reform had an impact on immigrant children's educational integration.

Based on our survey, we compute two educational outcome measures. The first is intended to capture children's school performance. In the German school system, grades vary discreetly from 1 (excellent) to 6 (insufficient), with grades below 3 being considered as achievements that exceed average requirements. In a first step, we calculate an average test score based on participants' self-reported grades in the subjects "German" and "Mathematics".[51] From this, we create the indicator *Above-Average Score* which equals one if a participant's average test score is better than 3, and is zero otherwise. The second outcome measure intends to capture parents' involvement in their children's learning. Specifically, we create the indicator *Parental Involvement* which equals one if a participant reports to receive parental support in homework, and is zero for those who do not obtain such support. Both in the broad and in the narrow sample, roughly 40% all children report above-average grades, while slightly more than 70% receive learning support from their parents.

We now once more run our preferred DiD specification, but with the variable *Trust Discrimination* replaced by the indicators *Above-Average Score* and *Parental Involvement*, respectively. Table 4.2 presents the results. We start by discussing the estimates for the school performance measure [Columns (BS.1) and (NS.1)]. The key message one may extract from Panel A, in which all regressions are run for boys and girls together, is that the citizenship reform substantially reduced the immigrant-native gap in school performance. For example, in the broad sample [Column (BS.1)], immigrant children born pre-policy are 9.9 percentage points less likely than their native peers to achieve above-average grades. The statistically significant DiD estimate of 8.9 percentage points suggests that the policy reduced this achievement gap by almost 90%. As should be expected, once we restrict our attention to the high-eligibility treatment group of immigrants with a Turkish background [Column (NS.1)], this effect becomes more pronounced, though somewhat less precisely estimated. In Panels B and C, in which the analysis is broken down by gender, it is evident that the reform effect is almost entirely driven by

[51] We have chosen an average test score as this constitutes a more comprehensive measure of academic achievement than a single-subject indicator.

male immigrants. For example, in Column (NS.1) of Panel B, we see that Turkish boys born pre-policy are 27.3 percentage points less likely than their native peers to achieve above-average grades, but the statistically significant (at the 1%-level) DiD estimate of 24 percentage points implies a reduction of this gap by 88%. For immigrant girls (see Panel C), we obtain a different picture: the pre-reform immigrant-native achievement gap ($\hat{\beta}_1$) among girls is much lower, both in the broad (-3.6 percentage points) and in the narrow sample (-4.4 percentage points). Loosely put, this implies that immigrant girls had much less to catch (educationally) than immigrant boys. Moreover, the coefficients on the DiD interaction are, tough positive, statistically indistinguishable from zero.

The remaining two columns of Table 4.2 use our measure of parental involvement as the dependent variable. Overall, we find that the introduction of *jus soli* led immigrant boys to receive more learning support from their parents. For example, in Column (BS.2) in Panel B, we find that immigrant boys born pre-policy are 33.8 percentage points less likely than their native peers to have parents who support their learning, but for those born post-policy the gap is a statistically significant 21.3 percentage points or 63% smaller. As before, this effect is more pronounced in the narrow sample: we observe a pre-reform immigrant-native gap in parental support of -30.5 percentage points and a policy-induced reduction thereof of 24.5 percentage points. For immigrant girls, the coefficients on the DiD interaction have the opposite sign but are not statistically different from zero.

Table 4.2: Birthright citizenship and education: diff-in-diff analysis

Dependent Variable: Trust Discrimination				
	Above-Average Score		Parental Involvement	
	(BS.1)	(NS.1)	(BS.2)	(NS.2)
Panel A: All				
Migrant ($\hat{\beta}_1$)	-0.099**	-0.123**	-0.194***	-0.147
	(0.040)	(0.054)	(0.053)	(0.086)
Born post-reform*Migrant ($\hat{\beta}_3$)	0.089**	0.128*	0.033	0.007
	(0.035)	(0.062)	(0.069)	(0.122)
Observations	1,256	1,057	1,262	1,060
R-squared	0.045	0.044	0.107	0.084
Panel B: Boys				
Migrant ($\hat{\beta}_1$)	-0.199***	-0.273***	-0.338***	-0.305***
	(0.056)	(0.063)	(0.075)	(0.074)
Born post-reform*Migrant ($\hat{\beta}_3$)	0.184***	0.240***	0.213**	0.245***
	(0.060)	(0.052)	(0.081)	(0.075)
Observations	604	517	610	521
R-squared	0.052	0.054	0.150	0.095
Panel C: Girls				
Migrant ($\hat{\beta}_1$)	-0.036	-0.044	-0.096*	-0.029
	(0.067)	(0.079)	(0.050)	(0.085)
Born post-reform*Migrant ($\hat{\beta}_3$)	0.014	0.056	-0.109	-0.219
	(0.048)	(0.106)	(0.073)	(0.153)
	652	540	652	539
R-squared	0.067	0.065	0.125	0.140
Month of Birth FE	Yes	Yes	Yes	Yes
Family Characteristics	Yes	Yes	Yes	Yes
Class Characteristics	Yes	Yes	Yes	Yes

Notes: OLS estimates of Equation (2). Standard errors clustered by school programme and location and reported in parentheses. All specification in Panel A control for gender. *Family characteristics* include mothers' age, dummy variables for mothers' education (eight groups) and dummy variables for family structure (five groups) . *Class characteristics* include class size, proportion of students with migration background, the gender ratio and five victimization measures (i.e., the proportion of students who report having been victims, in the past year, of physical abuse, verbal abuse, lies, theft and exclusion). *** (**) (*) indicates significance at the 1% (5%) (10%) level.

Let us interpret the findings to this point. For immigrant boys, there is a strong correlation between the educational gap separating them from their native peers and inter-group trust: before the citizenship reform took effect, they were lagging behind their native peers educationally and strongly discriminated against them in the trust game. The introduction of *jus soli*, in turn, saw a substantial reduction in trust discrimination

among immigrant boys together a with near-closure of the achievement gap between them and their native peers. Thus, one suggestive interpretation is that the immigrant-native gap in education is an important factor in immigrant boys' in-group/out-group behavior and a likely channel for changes thereof in the wake of the citizenship reform.

This explanation, however, leaves us with a puzzle regarding immigrant girls' trust decisions. In particular, in stark contrast to immigrant boys, immigrant girls were not lagging much behind their native peers educationally before the citizenship reform took effect. Despite this, we observe a substantial degree of trust discrimination among immigrant girls born pre-policy. Moreover, for them, the introduction of *jus soli* neither had a discernible effect on their in-group/out-group behavior nor was it fostering their educational integration. Overall, this suggests that education does not exert the same influence on immigrant girls as on immigrant boys, potentially because it is overridden by other identity-related (e.g., cultural) factors. In our final analytical step, we turn attention to this issue.

4.3.5.3 Immigrant Girls: What Explains their Strong and Persistent Out-Group Discrimination?

A consistent finding in many psychological and sociological studies of immigrant families is that parents adopt different socialization strategies for their daughters and their sons (for an insightful review, see Suárez-Orozco and Qin (2006)). In particular, across many immigrant groups, girls are socialized to be "keepers of the culture" and often face strict parental restrictions on extra-household activities that boys are free to choose (e.g., spend time with friends, got to parties, participate in after-school programs). Moreover, this double standard in parental control has been found to be particularly strong when immigrant parents perceive the host society as posing a threat to the values of their native culture (Dion and Dion, 2001).

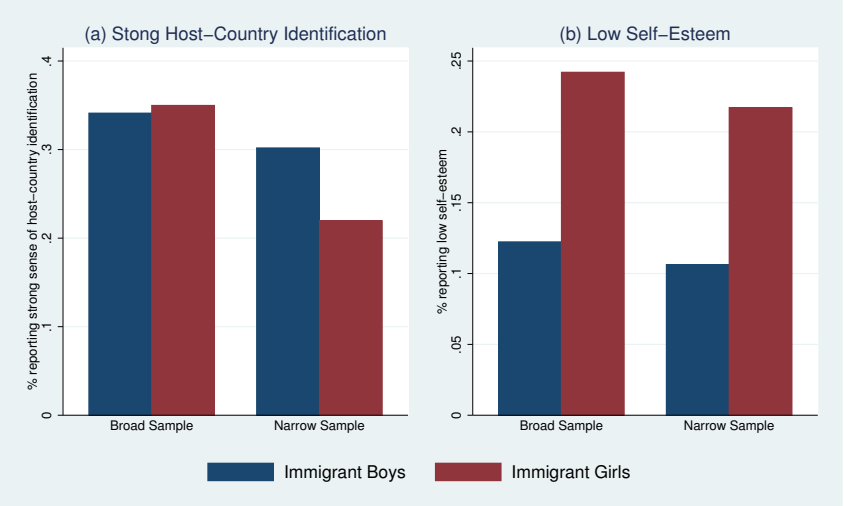
The existing literature suggests two implications of this gender-specific socialization pattern that may be relevant in our context. First, it shapes the process of ethnic self-identification. For example, in the United States, immigrant girls are more likely than immigrant boys to ethnically self-identify with their parents' immigrant origins; immigrant boys, by contrast, are more likely to choose a national identity (Qin-Hilliard,

2003; Yip and Fuligni, 2002). This, in turn, may influence the extent to which immigrant boys and girls differentiate between in-groupers and out-groupers. Second, due to the double standard in parental control, immigrant girls are more likely than immigrant boys to face a high level of parent-child conflict and, consequently, have lower self-esteem (Rumbaut, 1994). According to the "self-esteem hypothesis" in the psychological theory of social identity (Hogg and Abrams, 1990; Rubin and Hewstone, 1998) immigrant girls might restore a more positive self-concept through out-group discrimination.

Our survey allows us descriptively examine whether there are gender differences in immigrants' sense of identification with the host country and their self-esteem. Figure 4.8 presents the results. In Panel (a), we plot gender-specific proportions of second-generation immigrants reporting a strong sense of identification with their host nation.[52] In the broad sample, we detect no noticeable gender differences: roughly one-third of all immigrant boys and girls self-identify with their host nation. However, once we restrict our attention to Turkish immigrants in the narrow sample, we observe that roughly 30 percent of all Turkish boys self-identify with Germany, while the corresponding share among immigrant girls is one-third lower. Unconditional OLS regressions reveal this difference to be statistically significant at the 5% level. This finding ties in well with the above discussion: Turkish immigrants are predominately Muslim, and evidence suggests that their identification with the Islamic culture is strong—not just in Germany, but also in other major European destinations of Turkish migration, such as France and the Netherlands (Ersanilli and Koopmans, 2009). As argued above, such an environment typically reinforces differential socialization pressures on immigrant girls and boys.

[52]In particular, the panel is based on the following question: How much do you feel like a German (Very much, rather much, in some sense, not much, not at all)? Our outcome measure *Strong Host-Country Identification* equals one for participants who choose the answer categories "very much" or "rather much", and is zero otherwise.

Figure 4.8: Gender differences in immigrants' sense of host-country identification and self-esteem



In Panel (b), we plot gender-specific proportions of second-generation immigrants reporting a low level of self-esteem.[53] In both the broad and the narrow sample, roughly one-tenth of all immigrant boys report a low level of self-esteem, while the corresponding share among immigrant girls is twice as a large. Unconditional OLS regressions show the gender differences to be statistically significant at the 1% level. Moreover, the results hold for several alternative measures for self-esteem. Taken as whole, we view this as suggestive evidence that immigrant boys show better psychological adaption than do immigrant girls, a finding also reached in a recent cross-country study of offspring of Turkish and Viatnamese immigrants (Berry *et al.*, 2006). To the extent that low-self esteem can reinforce inter-group discrimination, we also view it as a possible explanation for the strong and persistent in-group/out-group bias shown by immigrant girls.

4.4 Conclusion

Immigration has put many developed countries on a new demographic path. Immigrant children, in particular, make up a large and growing proportion of youth populations around the western world. As a result, many scholars and policy makers argue that

[53]In our survey, we asked participants: Do what extent does the following statement apply to you [on a discrete scale from 1 (does not apply to all) to 6 (applies completely)]: I have a positive attitude towards myself. Our outcome measure *Low Self-Esteem* equals one for participants who place themselves in the bottom half of the six-point scale, and is zero otherwise.

success in integrating immigrant children will be a crucial nation-building tool for years to come. Our starting point was the recognition that integration has many dimensions. In particular, if we are to get a glimpse of the future face of western societies, it is not just important to understand how today's immigrant children fare in the education system, but we also need to know how children with and without migrational backgrounds socially interact and whether integration interventions can bring about cooperation between. We have addressed this issue by combining a natural experiment—the introduction of birthright citizenship in Germany—with an artefactual field experiment based on the trust game with almost 4,500 adolescents in their final year of compulsory schooling.

Based on the artefactual field experiment, we have found empirical evidence for an oppositional culture of trust in Germany: native children extent their trust in almost equal measure to native and immigrant opponents, respectively. Immigrant children, by contrast, show a high degree of trust towards those with whom they share a migration background, but a low willingness to trust their native peers. This result makes cleavages between immigrants and natives in Germany apparent.

In connecting the experiment to the introduction of birthright citizenship, we have shown that these cleavages are not set in stone: the policy substantially reduced the degree of trust discrimination among male, but not among female, immigrant children. Moreover, there are convincing reasons to believe that the main channel for the effect of birthright citizenship is through an improved educational integration of immigrant males. Thus, we conclude that granting citizenship to all children born on a nation's territory, irrespective of their parents nationality, can have a strong positive effect on the social interaction between children with and without migrational backgrounds.

That said, the results also point to an important, open challenge for policy makers: we have found immigrant girls to strongly discriminate in their trust decisions against their native peers; yet, the positive reform effects we have uncovered—both in terms of cross-group trust and education—are an entirely male phenomenon. This suggests that integration interventions targeted at immigrant children may need to be gender-specific and take into account the different socialization pressures that immigrant girls and boys experience in the process of assimilation.

Appendix C

Anleitung

Deine Teilnahme ist freiwillig!

Vielen Dank für deine Teilnahme! Bitte sprich von nun an mit niemandem außer mit uns! Leider musst du sonst aus dem Spiel genommen werden.

Wir wollen in diesem Spiel untersuchen, wie sich Menschen entscheiden. Es gibt kein "Richtig" oder "Falsch" in diesem Spiel, und wir wollen auch nicht dein Wissen überprüfen. Triff deine Entscheidungen, wie du sie gerne treffen möchtest. Du wirst dabei echtes Geld verdienen. Wir garantieren, dass dir das Geld in spätestens zwei Wochen in bar ausbezahlt wird. Du erhältst es in einem Kuvert, auf dem deine ID-Nummer steht. Bewahre daher deine ID-Nummer sorgfältig auf! Die Kuverts werden von einem deiner Lehrer verteilt bzw. können im Sekretariat abgeholt werden.

Wie viel Geld du verdienst, hängt von deinen Entscheidungen und von den Entscheidungen anderer Spieler ab. Die genauen Regeln erklären wir dir jetzt. Es ist daher besonders wichtig, dass du jetzt aufmerksam zuhörst.

Es gibt in diesem Spiel keine richtigen oder falschen Entscheidungen. Triff deine Entscheidungen nach deinen eigenen Überlegungen! Deine Entscheidungen werden anonym bleiben, das heißt, niemand wird davon erfahren.

Wenn du nach dem Vorlesen dieser Anleitung Fragen hast, hebe bitte die Hand. Wir werden dann zu dir kommen und die Fragen privat (das heißt: leise) beantworten.

Ablauf:

In diesem Spiel gibt es zwei Rollen: **Sender** und **Rücksender**.

So beginnt das Spiel: Jeder Sender und jeder Rücksender erhält jeweils 5 EURO. Der Sender muss entscheiden, wie viel er von den 5 EURO an den Rücksender abgibt.

Die Summe, die der Sender an „seinen" Rücksender abgibt, wird dann von uns verdreifacht. Das heißt, der Rücksender bekommt genau dreimal so viel, wie ihm der Sender abgegeben hat.

Danach ist der Rücksender an der Reihe. Er besitzt jetzt den verdreifachten Betrag, den der Sender ihm abgegeben hat, plus seine eigenen 5 EURO. Der Rücksender muss jetzt

entscheiden, wie viel er davon an „seinen“ Sender zurückgeben will. Bitte beachte: Die Summe, die der Rücksender an den Sender zurückgibt, wird von uns nicht verdreifacht.

Auszahlung:

Der Sender erhält am Schluss den Betrag, den er nicht abgegeben hat, plus jenen, den der Rücksender ihm zurückgegeben hat.

Auszahlung Sender = 5 EURO – gesendeter Betrag + rückgesandter Betrag (durch Rücksender)

Der Rücksender erhält die 5 Euro und den Betrag, den er vom Sender erhalten hat (mal 3), minus dem, was er an den Sender zurückgegeben hat.

Auszahlung Rücksender = 5 EURO + 3 x gesendeter Betrag (durch Sender) – rückgesandter Betrag

Entscheidungen:

Du entscheidest einmal in der Rolle des Senders und einmal in der Rolle des Rücksenders. Außerdem kannst du zwischen verschiedenen „Personengruppen“ von Sendern und Rücksendern unterscheiden; du musst diese aber natürlich nicht unterschiedlich behandeln. Auf dem Entscheidungsblatt werden diese Personengruppen beschrieben. Zum Beispiel wird unterschieden werden, ob du einem Mädchen oder einem Jungen Geld sendest oder es an ihn bzw. sie zurücksendest. Die Entscheidung liegt bei dir, und es gibt kein “Richtig” oder “Falsch”.

Bestimmung Deiner Auszahlung:

Einige der folgenden Punkte werden einfacher zu verstehen sein, nachdem du die Entscheidungsblätter gesehen hast. Wir gehen die Punkte jetzt durch, sehen uns dann gemeinsam die Entscheidungsblätter an, und wenn es dann noch Fragen gibt, können wir gerne wieder zu diesen Punkten zurückkehren. Nach unseren Spielen an mehreren Schulen wird Folgendes passieren:

1. Es werden zufällig zwei Schüler aus verschiedenen Schulen zusammengewürfelt; du kennst also „deinen“ Sender oder „deinen“ Rücksender nicht persönlich; Er oder sie ist aber in etwa gleich alt ist wie du und geht in Schleswig-Holstein zur Schule.

2. Es wird zufällig bestimmt, wer die Rolle des Senders übernimmt und wer die Rolle des Rücksenders übernimmt.

3. Danach wird bestimmt, aus welcher Personengruppe (siehe Entscheidungsbogen) der Sender und der Rücksender jeweils kommen. Das wissen wir aufgrund deiner Angaben im Fragebogen. Zum Beispiel kann der Sender ein Mädchen sein und der Rücksender ein Junge.

4. Dann wird die Entscheidung des Senders für die tatsächliche Personengruppe des Rücksenders umgesetzt.

5. Schließlich wird die Entscheidung des Rücksenders für die tatsächliche Personengruppe des Senders und für den von „seinem“ Sender tatsächlich gesendeten Betrag umgesetzt.

6. Wir wissen nun, was der Sender gesendet hat und was der Rücksender zurückgesendet hat und können daraus die Auszahlung des Sender und des Rücksenders berechnen. Dann legen wir das Geld in die entsprechenden Kuverts mit den jeweiligen ID-Nummern des Senders und des Rücksenders und bringen es an die Schulen.

7. Im Anschluss daran kannst du deine Auszahlung in einem Kuvert an deiner Schule abholen.

Sieh dir nun die Entscheidungsblätter an – dann werden einige Punkte leichter zu verstehen sein. Überlege dir gut, welche Entscheidungen du treffen möchtest. Du hast genügend Zeit dafür! Wenn du Fragen hast, heb bitte deine Hand! Jemand wird dann zu dir kommen und die Frage privat (das heißt: leise) beantworten.

Figure C.1: Photo of a classroom before starting the survey

Table C.1: Birthright citizenship and trust discrimination: robustness - discriminator

Dependent Variable: Trust Discrimination						
	Alternative TD		Discriminator		Strong discriminator	
	(BS.1)	(NS.1)	(BS.2)	(NS.2)	(BS.3)	(NS.3)
Panel A: All						
Migrant ($\hat{\beta}_1$)	0.300***	0.459***	0.164**	0.242**	0.135***	0.173**
	(0.059)	(0.130)	(0.062)	(0.099)	(0.043)	(0.079)
Born post-reform*Migrant ($\hat{\beta}_3$)	-0.112	-0.216**	-0.039	-0.084	-0.035	-0.058
	(0.082)	(0.077)	(0.063)	(0.080)	(0.062)	(0.085)
Observations	1,280	1,078	1,280	1,078	1,280	1,078
R-squared	0.05	0.05	0.06	0.06	0.052	0.05
Panel B: Boys						
Migrant ($\hat{\beta}_1$)	0.262*	0.381**	0.151*	0.267**	0.136*	0.204**
	(0.130)	(0.172)	(0.080)	(0.105)	(0.069)	(0.089))
Born post-reform*Migrant ($\hat{\beta}_3$)	-0.282**	-0.508**	-0.098	-0.194**	-0.086	-0.208**
	(0.125)	(0.211)	(0.067)	(0.077)	(0.074)	(0.082)
Observations	618	529	618	529	618	529
R-squared	0.07	0.07	0.07	0.08	0.05	0.06
Panel C: Girls						
Migrant ($\hat{\beta}_1$)	0.370***	0.575***	0.175**	0.238*	0.139***	0.170*
	(0.057)	(0.176)	(0.072)	(0.114)	(0.036)	(0.080)
Born post-reform*Migrant ($\hat{\beta}_3$)	0.029	-0.020	0.013	-0.015	0.010	0.038
	(0.108)	(0.186)	(0.064)	(0.082)	(0.064)	(0.081)
Observations	662	549	662	549	662	549
R-squared	0.10	0.13	0.09	0.09	0.10	0.10
Month of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Family Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Class Characteristics	Yes	Yes	Yes	Yes	Yes	Yes

Notes: OLS estimates of equation (2). Pre-reform difference refers to the *conditional* pre-reform difference in trust discrimination between treatment and control group. Standard errors clustered by school programme and location and reported in parentheses. All specification in panel A control for gender. *Family characteristics* include mothers' age, dummy variables for mothers' education (eight groups) and dummy variables for family structure (five groups) . *Class characteristics* include class size, proportion of students with migration background, the gender ratio and five victimization measures (i.e., the proportion of students who report having been victims, in the past year, of physical abuse, verbal abuse, lies, theft and exclusion). *** (**) (*) indicates significance at the 1% (5%) (10%) level.

Table C.2: Birthright citizenship and trust discrimination: robustness - specification

Dependent Variable: Trust Discrimination						
	t-wild cluster		3-month window		Placebo reform	
	(BS.4)	(NS.4)	(BS.5)	(NS.5)	(BS.6)	(NS.6)
Panel A: All						
Migrant ($\hat{\beta}_1$)	0.337***	0.497***	0.330***	0.490***	0.335**	0.430**
	[0.000]	[0.000]	(0.085)	(0.154)	(0.121)	(0.189)
Born post-reform*Migrant ($\hat{\beta}_3$)	-0.106	-0.254**	-0.069	-0.210*	0.048	0.014
	[0.263]	[0.020]	(0.095)	(0.113)	(0.158)	(0.149)
Observations	1,280	1,078	961	800	557	473
R-squared	0.06	0.06	0.07	0.07	0.112	0.13
Panel B: Boys						
Migrant ($\hat{\beta}_1$)	0.281**	0.459**	0.226	0.505**	0.356**	0.420
	[0.040]	[0.020]	(0.146)	(0.238)	(0.161)	(0.257)
Born post-reform*Migrant ($\hat{\beta}_3$)	-0.260**	-0.565**	-0.214	-0.612**	-0.035	-0.002
	[0.040]	[0.020]	(0.146)	(0.222)	(0.201)	(0.270)
Observations	618	529	461	390	265	232
R-squared	0.06	0.07	0.08	0.10	0.15	0.15
Panel C: Girls						
Migrant ($\hat{\beta}_1$)	0.418***	0.586***	0.454***	0.547***	0.352**	0.476**
	[0.000]	[0.000]	(0.085)	(0.154)	(0.149)	(0.196)
Born post-reform*Migrant ($\hat{\beta}_3$)	0.034	-0.056	0.027	0.007	0.114	0.010
	[0.667]	[0.889]	(0.105)	(0.161)	(0.171)	(0.126)
Observations	662	549	500	410	292	241
R-squared	0.12	0.14	0.15	0.16	0.14	0.18
Month of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Family Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Class Characteristics	Yes	Yes	Yes	Yes	Yes	Yes

Notes: OLS estimates of equation (2). Pre-reform difference refers to the *conditional* pre-reform difference in trust discrimination between treatment and control group. Standard errors clustered by school programme and location and reported in parentheses. For the specifications based on t-wild cluster bootstrap procedures, we report p-values in square brackets. All specification in panel A control for gender. *Family characteristics* include mothers' age, dummy variables for mothers' education (eight groups) and dummy variables for family structure (five groups) . *Class characteristics* include class size, proportion of students with migration background, the gender ratio and five victimization measures (i.e., the proportion of students who report having been victims, in the past year, of physical abuse, verbal abuse, lies, theft and exclusion). *** (**) (*) indicates significance at the 1% (5%) (10%) level.

Appendix D

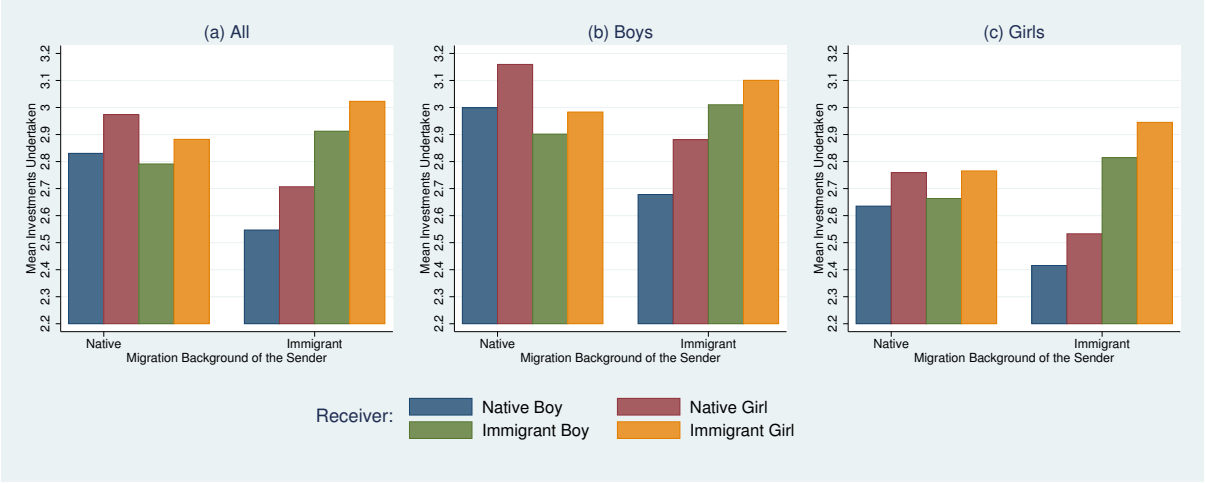
Table D.1: Descriptives for the full sample

	Natives	Immigrants
Gender and Family Structure		
Female	0.536	0.500
Lives with both parents	0.547	0.738
Lives with one parent	0.381	0.172
Lives with: other	0.027	0.024
Lives with: missing	0.044	0.066
Mother's Age	46.124	44.102
Mother's Education		
None or Low ("Hauptschule")	0.248	0.324
Intermediate ("Realschule")	0.412	0.173
High ("Abitur") or University	0.206	0.169
Other, Unknown or Missing	0.134	0.334
Religious Affiliation		
Catholic	0.141	0.163
Protestant	0.525	0.085
Islamic	0.018	0.590
None, Other, Missing	0.316	0.162
Language Spoken at Home		
Mostly German	0.976	0.289
Mostly Foreign Language	0.011	0.690
Missing	0.013	0.021
Mother's Country/Region of Birth		
Turkey	/	0.376
Middle East & Africa	/	0.139
Post-Soviet Country	/	0.123
Balkan Country	/	0.112
Eastern Europe	/	0.106
Other Country	/	0.144
Observations	2,201	1,218

Notes: "Natives" are children whose parents are both German-born. "Immigrants" are children whose parents are both foreign-born.

Appendix E: Trust Conditional on Migration Background and Gender

Figure E.1: Investments of native and immigrant children by migration background and gender of second movers



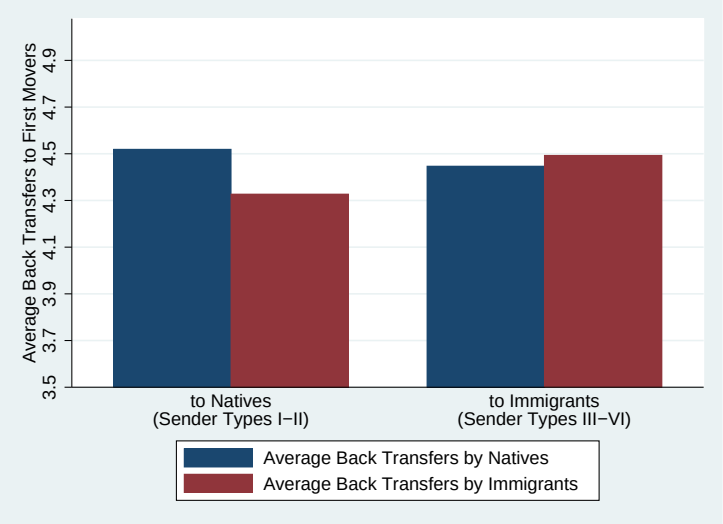
Appendix F: Which Types of Discrimination Can Explain Observed Behavior

In this Appendix, we provide an in-depth discussion of two questions related to second mover behavior and expectations. First, do children with and without migrational background receive lower back transfers from in-group opponents than from out-group opponents. Second, are trust decisions vis-a-vis players of different migrational backgrounds mirrored in differential expectations of reciprocity? Answering the first question allows determining whether there is statistical discrimination in the trust game. Our results with regard to the third question shed some light on the role of discrimination based on stereotypes.

Back Transfers: Are Discriminatory Trust Decisions Statistically "Justified"

In their seminal paper on trust in the segmented Israeli society, Fershtman and Gneezy (2001) pointed out that ethnic trust discrimination may be rational when a comparison of different ethnic groups reveals systematic statistical differences with respect to reciprocal behavior. In a similar vein, in our study, the discriminatory trust decisions of children with migrational backgrounds could be considered as justified on statistical grounds if immigrant children receive lower back transfers from native children than from other immigrants. Figure F.1 provides some insights into this issue. It shows the average of the eleven contingent back transfers of native and immigrant children to different types of first movers. The main results are as follows. The first pair of bars shows that the average back transfers of native children to native first mover types (4.51 €) exceed the average back transfers of immigrant children to native first mover types (4.31 €) by a statistically significant 4.6% (two-sample t-test, $p < 0.05$). Appendix Figure F.2 demonstrates that this result applies equally if we consider the average back transfers of native and immigrant children to native boys and girls, respectively. Thus, a possible interpretation is that the first mover in-group/out-group investment gap of 4.8% among native boys (see Figure 4.2 above) reflects discrimination against immigrants on statistical grounds. The second pair of bars indicates that the average back transfers of native children to immigrant first mover types (4.43 €) are almost identical to the average back transfers

Figure F.1: Averages of the 11 contingent back transfers



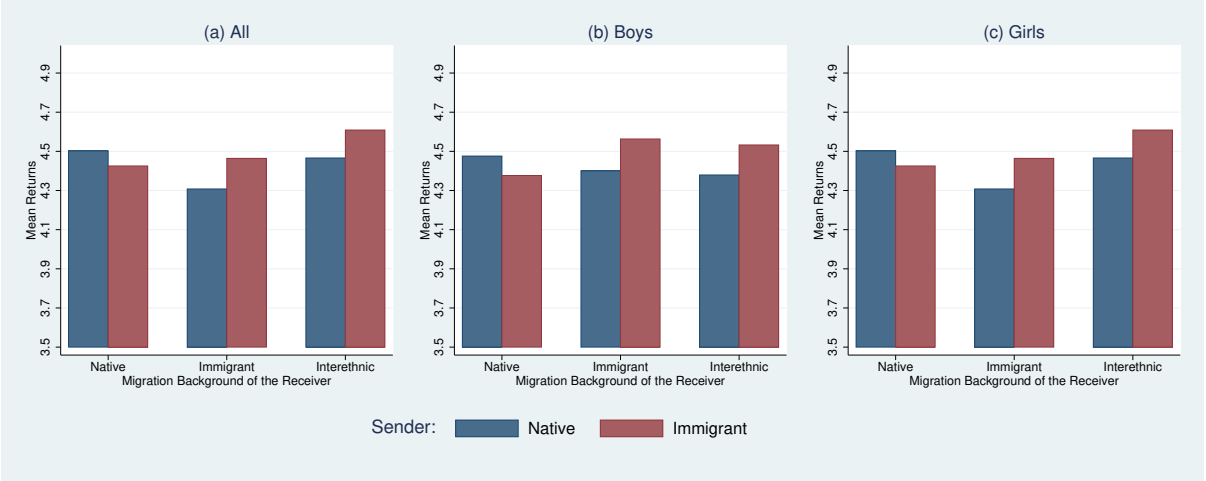
Notes: Average back transfers to natives are defined as the mean of the average of the 11 contingent back transfers to sender types I-II. Average back transfers to immigrants are defined as the mean of the average of the 11 contingent back transfers to sender types III-VI.

of immigrant children to immigrant first mover types (4.47€), with the difference being statistically indistinguishable from zero and the pattern not being dependent on the gender of immigrant first movers (see Appendix Figure F.2). We conclude, therefore, that the discriminatory trust decisions of immigrants are not a reflection of statistical discrimination.

Expectations: Are Discriminatory Trust Decisions Based on Wrong Stereotypes?

Although immigrant children do not receive lower back transfers from natives than from immigrants, they may have the wrong perception that they do. If this were the case, the trust decisions of immigrants vis-a-vis natives could be interpreted as discrimination based on wrong stereotypes. In our experiment, first movers indicated on their decision sheet the expected back transfer from the six possible receiver types. Figure F.3 conditions on own investments as first movers, and shows how much native and immigrant children expect to receive back from native and immigrant receiver types, respectively. Panel (a) reveals that native children have the (correct) expectation that they will receive higher back transfers from native than from immigrant receiver types. By contrast, Panel (b)

Figure F.2: Average of the 11 contingent back transfers by migration background of second movers and gendered first mover types



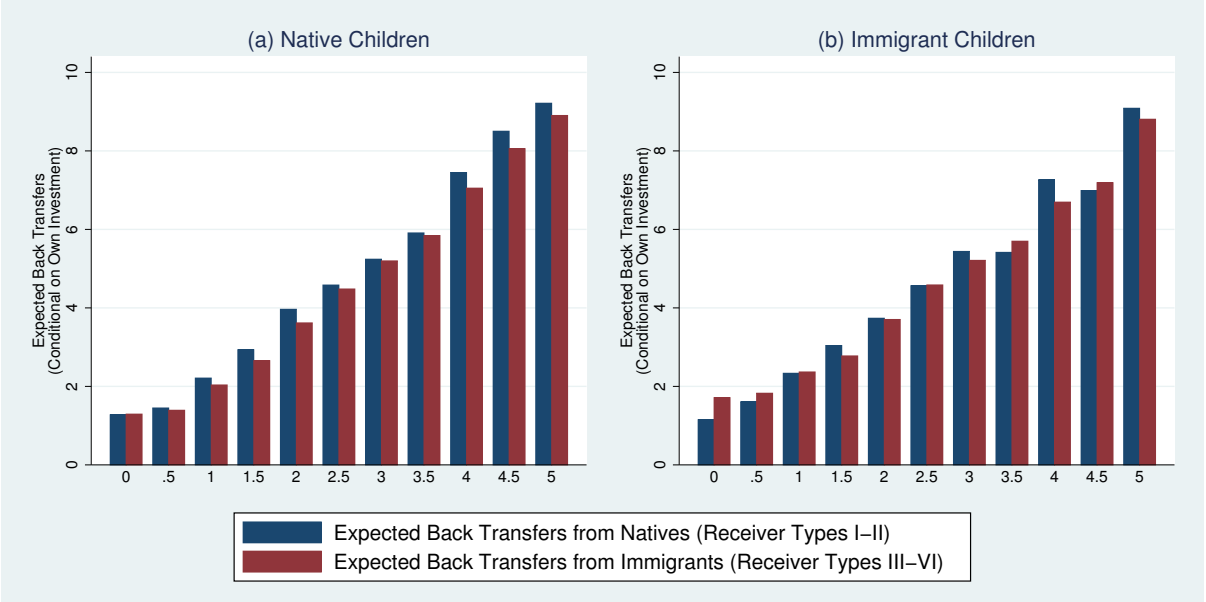
Notes: Average back transfers to natives are defined as the mean of the average of the 11 contingent back transfers to sender types I and II. Average back transfers to immigrants are defined as the mean of the average of the 11 contingent back transfers to sender types I and II.

indicates that immigrant children do not systematically expect that they will receive lower back payments from native than from immigrant receiver types. Last, Appendix Figure F.4 shows that there are no noticeable gender differences in the just-described patterns. We therefore conclude that discrimination based on wrong stereotypes does not drive the differential trust decisions of immigrants. Since our results on repayment behavior also speak against statistical discrimination, the mistrust of immigrants toward natives is probably due to a "taste for discrimination".

According to Becker (1957), taste-based discrimination arises from prejudices whereby individuals are willing to pay a price to discriminate. This aspect of taste-based discrimination is also apparent in our experiment: the discriminatory trust decisions of immigrants involve a sacrifice of money. Table F.1 illustrates this point. It first shows the expected payoffs of first movers when we, in a thought experiment, randomly match them with in-group and out-group opponents, respectively. The evidence indicates that first movers do better in terms of expected payoffs when matched with an in-group rather than an out-group opponent, a result that holds for both natives and immigrants.[54]

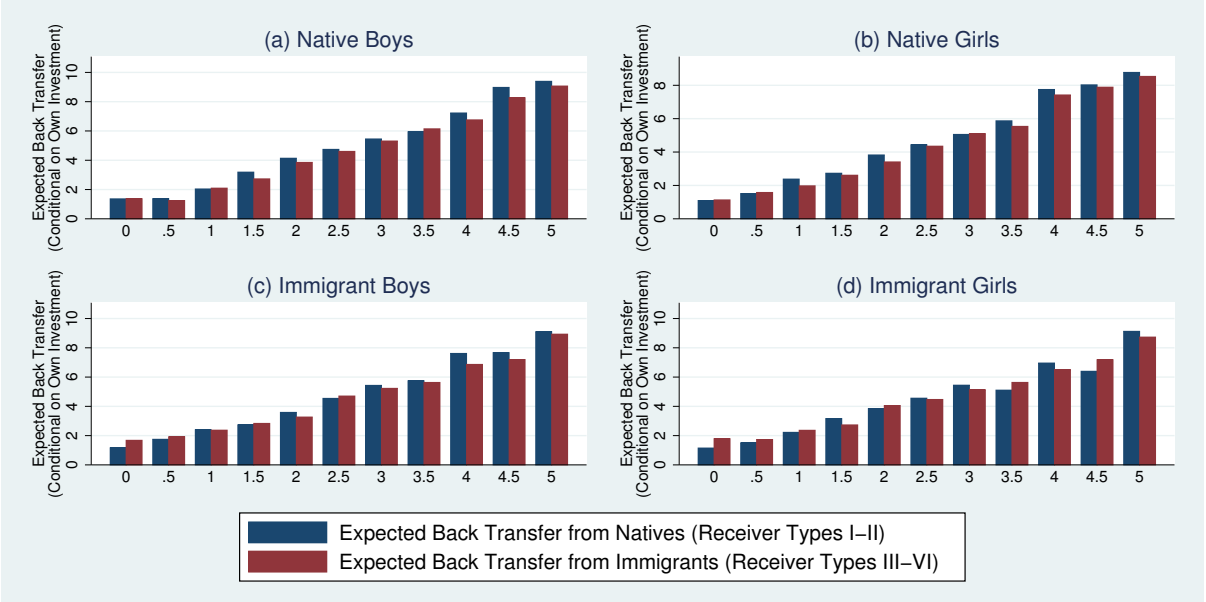
[54]Figure F.5 shows the expected payoffs of first movers as a function of their own investments according to the migration background and gender of second movers. For both native and immigrant first movers, we observe that their expected payoffs are strictly increasing functions of own investments, a result that holds irrespective of the migration background and gender of second movers. This supports the notion that the discriminatory trust decisions of immigrants vis-a-vis natives involves payoff losses.

Figure F.3: Expected returns on investment



The table then goes on to provide back-of-the-envelope calculations of the shares of the in-group/out-group payoff gaps that are due to own discriminatory trust decisions (as opposed to the shares of the payoff gaps that are due to reciprocity discrimination by second movers). For immigrants, we observe that the in-group/out-group payoff gap is almost entirely explained by their own discriminatory trust decisions vis-a-vis natives (96% of the gap for boys, 84% of the gap for girls). For natives, by contrast, only a small share of the in-group/out-group payoff gap stems from their own discriminatory trust decisions (16% of the gap for boys, 0% of the gap for girls). This, in turn, implies that their monetary losses when matched with out-group opponents can to a large extent be explained by the discriminatory repayment choices of immigrants. To summarize, the findings indicate that immigrants' mistrust toward natives is likely due to a taste for discrimination and appears to be reflected in a willingness to sacrifice money.

Figure F.4: Conditional expected back transfers by gender



Appendix G:Mixed-Background Children and Second-Generation Immigrant Children

Figure G.1: Investments of mixed-background children by migration background of second movers

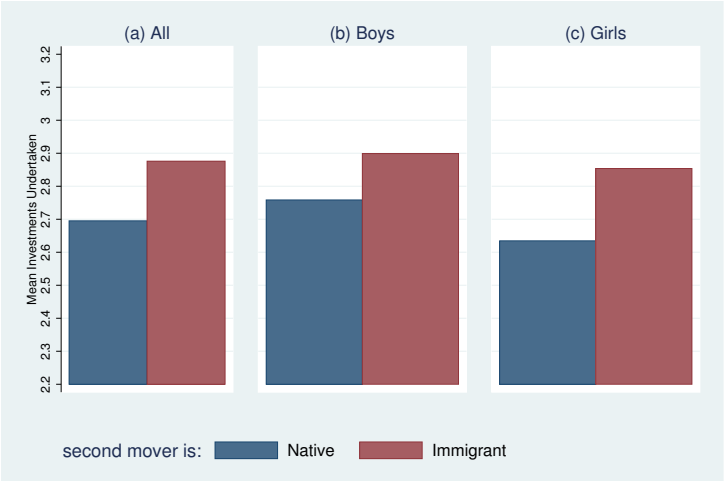


Table F.1: Payoff losses due to trust discrimination

	Natives			Immigrants		
	All	Boys	Girls	All	Boys	Girls
Expected in-group payoffs	7.27	7.38	7.13	7.22	7.23	7.21
Expected out-group payoffs	6.99	7.06	6.90	7.04	7.09	6.99
In-group/out-group payoff gap	**0.28**	**0.32**	**0.23**	**0.18**	**0.14**	**0.22**
Marginal out-group payoffs	0.35	0.36	0.34	0.47	0.48	0.46
In-group/out-group investment gap	0.07	0.14	-0.02	0.35	0.28	0.40
% of the in-group/out-group payoff gap due to trust discrimination	**8.8%**	**15.8%**	**0%**	**91.4%**	**96%**	**83.6%**

Notes: *Expected in-group payoffs* (respectively, *expected out-group payoffs*) are the payoffs a participant can expect when randomly matched to second mover of his or her own migration background (respectively, a second mover that is not of his or her own migrations background). *Marginal out-group payoffs* are the slopes between expected out-group payoffs and out-group investments estimated from OLS regressions. The *share of the in-group/out-group payoff gap due to trust discrimination* is therefore the ratio of the product between marginal out-group payoffs and the in-group/out-group investment gap to the in-group/out-group payoff gap.

Figure G.2: Investments of first- and second-generation immigrant children by migration background of second movers

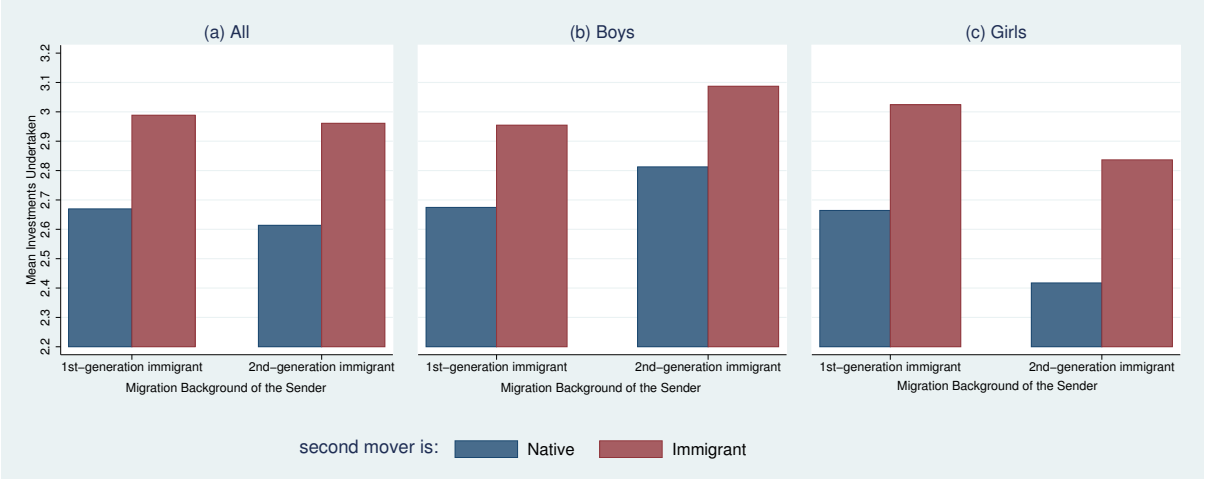
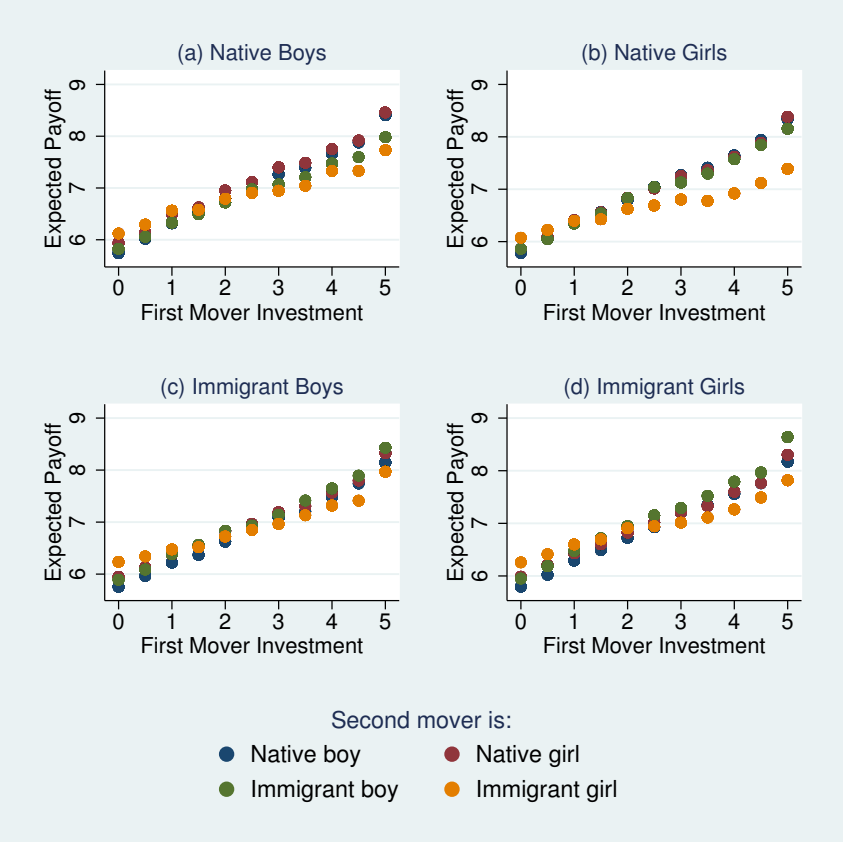


Figure G.3: Investments of first- and second-generation immigrant children, by migration background of second movers

Figure F.5: Expected payoffs of first movers as functions of their own investments and according to their own and their opponents' migration backround.



Appendix H: Descriptives for the Natural Experiment Samples (Broad and Narrow)

Table H.1: Descriptive for the broad NE sample

	Natives	Immi-grants	p-value	Pre-Reform	Post-Reform	p-value
			Gender and Family Structure			
Female	.499	.564	.005	.612	.521	.004
Lives with both Parents	.570	.808	.000	.824	.795	.419
Lives with Mother	.321	.103	.000	.100	.105	.796
Lives with Father	.048	.022	.004	.012	.032	.229
Lives with: Other	.014	.022	.225	.029	.016	.324
Lives with: Missing	.048	.044	.719	.035	.053	.368
Mother's Age	46.057	44.542	0	45.424	43.753	.009
			Mothers' Education			
None	.010	.172	.000	.165	.179	.566
Low	.198	.144	.043	.159	.132	.456
Intermediate	.432	.197	.000	.165	.226	.146
High	.149	.092	.049	.088	.095	.828
University	.077	.050	.042	.059	.042	.426
Other	.005	.025	.011	.041	.011	.148
Unknown	.100	.292	.000	.288	.295	.891
Missing	.029	.028	.861	.035	.021	.359
			Class Characteristics			
Class Size	19.186	17.903	.021	17.924	17.884	.936
Share Immigrants	.337	.569	.000	.562	.574	.695
Share Males	.489	.500	.293	.503	.498	.643
Share Insulted	.724	.702	.061	.696	.708	.272
Share Ignored	.503	.473	.004	.460	.485	.223
Share Hurt	.098	.116	.249	.119	.114	.470
Share Lied	.827	.738	.027	.716	.759	.052
Share Stolen	.217	.242	.016	.239	.245	.632
Observations	920	360		170	190	

Notes: Sample restricted to a 8-month window centered around the reform's cut-off date and excluding a 4-week window around the reform's cut-off date. "Natives" comprises children whose parents are both German-born. "Immigrants" (1) refers to children who are German-born, but whose parents are both foreign-born (second generation immigrants). "Pre-reform" and "Post-reform" refers to "Immigrants" who are either born before (in 1999) or after (in 2000) the reform's cut-off date. P-values refer to the respective differences between the groups.

Table H.2: Descriptives for the narrow NE sample

	Natives	Immi-grants	p-value	Pre-Reform	Post-Reform	p-value
			Gender and Family Structure			
Female	.499	.57	.005	.59	.55	.49
Lives with both Parents	.57	.829	0	.808	.85	.251
Lives with Mother	.321	.076	0	.103	.05	.024
Lives with Father	.048	.013	.004	.013	.013	.987
Lives with: Other	.014	.025	.225	.026	.025	.97
Lives with: Missing	.048	.057	.719	.051	.063	.74
Mother's Age	46.057	44.335	0	45.654	43.05	.055
			Mothers' Education			
None	.01	.266	0	.231	.3	.147
Low	.198	.19	.043	.205	.175	.624
Intermediate	.432	.139	0	.115	.163	.327
High	.149	.044	.049	.051	.038	.684
University	.077	.019	.042	.013	.025	.625
Other	.005	.038	.011	.051	.025	.55
Unknown	.1	.285	0	.295	.275	.811
Missing	.029	.019	.861	.038	0	.022
			Class Characteristics			
Class Size	19.186	17.411	.021	17.705	17.125	.437
Share Immigrants	.337	.591	0	.59	.592	.959
Share Males	.489	.494	.293	.5	.49	.559
Share Insulted	.724	.694	.061	.697	.69	.601
Share Ignored	.503	.447	.004	.443	.451	.766
Share Hurt	.098	.107	.249	.11	.104	.502
Share Lied	.827	.722	.027	.691	.752	.075
Share Stolen	.217	.243	.016	.237	.25	.628
Observations	920	158		78	80	

Notes: Sample restricted to a 8-month window centered around the reform's cut-off date and excluding a 4-week window around the reform's cut-off date. "Natives" comprises children whose parents are both German-born. "Immigrants" (1) refers to children who are German-born, but whose parents are both foreign-born, with at least one parent being of Turkish origin. "Pre-reform" and "Post-reform" refers to "Immigrants" who are either born before (in 1999) or after (in 2000) the reform's cut-off date. P-values refer to the respective differences between the groups.

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